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THE TILBURY DEEP WATER DOCKS.

WE publish an engraving giving a bird's eye view of the works as now being carried out, a large proportion of the work having been done, and showing that the original plans have been adhered to. The first turf was cut 8th July, 1882; the work is being carried out by the East and West India Dock Company. Contracts for the work were let to and commenced by one firm of contractors, but they are now being carried out vigorously by Messrs. Lucas and Aird, under Messrs. Manning and Baines, MM.I.C.E., who are the engineers of the undertaking. The entrance, which will be 100 yards wide, is opposite Gravesend. This leads into a tidal basin where ships can enter at all states of the tide, and discharge their passengers close by the large hotel which is now being built. The entrance lock is 80 ft. wide, with walls composed of Portland cement concrete, faced below water with stock brick, above pumping, there is a system of leveling culverts running from the bottom of each dry dock, so in the event of a vessel in No. 1 dry dock having finished repairs, and wanting to go out, and a vessel wanting to be docked in No. 4 dock, a large amount of water from No. 4 can be run out at low tide, and nearly all the remainder can be run off into No. 1, thus saving a large item in the cost of pumping.

There will be three caissons to the large and three to the small graving docks. These are built by Messrs. Green, of Blackwall. There will be three stops for the center caisson, to enable the length of the dry docks to be altered to suit the varying lengths of vessels. They are ship caissons, worked with air chambers and water tanks, to prevent their jumping. The cloughs of the graving docks will be worked by hand, so as to avoid the results of accidental leakage of water under pressure past the valves of a hydraulic ram, and thus lifting the cloughs at a wrong time. The walls of the main

THE NEW DEEP WATER DOCKS AT TILBURY.

water with blue brick. The quoins and pointing sills are all of Cornish granite. The gates are of wrought iron, and are being made by Messrs. Clayton and Co., Preston. A special arrangement is being adopted to carry the center of gravity of gates over the center of the rollers, the latter being large, and mounted in such a manner that they may be raised to the top of the gates, with their bearings, for inspection and repair, and so that they may be adjusted so as to carry each its due load, and to trim the gates. These will be worked by hydraulic rams placed in a horizontal position. All the hydraulic machinery will be supplied by Messrs. Armstrong and Co., with the exception of the cranes, which are being made by Messrs. Walker and Co., of Leeds. To the east of the lock are four large graving docks, the walls of which are built of concrete faced, where there is no wear and tear, with stock bricks. The altars are of York stone with blue bricks at the back. A timber coping will run round the graving docks, with eyebolts let in every few feet. The flooring of graving docks will be of pitch pine, and a dry pathway will lead round bottom of docks, to enable captains of vessels to examine the ships' bottoms without wetting their feet. The graving docks will be filled, either from the tidal basin or main dock, through large culverts, through which they can also be run cut to low water. The remaining water will be pumped out by large centrifugal pumps—a notice of which appeared in *The Engineer*, vol. lviii., p. 452. There will be one pump to each of the four graving docks, two of their suctions being 7 ft. in diameter and two 6 ft. diameter. These pumps are so arranged that any can be made to pump any one dock, so that it will be impossible for any delay with the work in docking. To save

and branch docks are similar to those of the lock, but with the exception that below water they are faced with fine concrete, instead of stock brick. The copings will be of granite. There will be a series of sheds down each jetty, and movable hydraulic cranes will run up and down the quays.

The hydraulic machinery will be worked from the engine-house at the north end of the docks. There will be three pairs of compound engines, made by Messrs. Armstrong, and the pressure pipes from these will make a complete circuit round all the dock walls. There will be a new station on the London, Tilbury, and Southend railway close by the engine-house, and those using the docks will walk along an overhead gangway over the various lines and down brows leading to the various docks. The goods will be brought by rail at the north end of the ground by a short length of railway now being made. Two canteens are being built, which will make ample provision for those using the docks.

A large quantity of the work, of which this is a brief account, is in a very forward state, a very large number of men and plant being employed. The works are of unusual magnitude, and there are many features in the construction of the walls, the arrangement and proportions of the culverts, the arrangements for controlling the level of the water in the docks and for pumping by any or all of the machinery from any of the docks, and of many other interesting engineering features.—*The Engineer.*

A LARGE steel manufacturer in Pittsburg, Pa., thinks that a year hence not a keg of iron nails will be made east of the Mississippi River.

STEEL BOILER PLATES.

By MR. W. PARKER.*

AN ordinary cylindrical boiler of 13 ft. diameter and 16 ft. long, designed for a pressure of 150 lb. per square inch, for which the scantlings were amply sufficient, burst under the hydraulic test. The pressure was applied very carefully, and when it had reached 240 lb. the fracture occurred, extending completely across one of the shell plates, and to a slight extent also into the adjoining plate, as shown on the diagram. The boiler was constructed entirely of steel, made on the Siemens-Martin process by a firm who enjoy the reputation of producing a material second to none in the country. The plates were all tested at the steel works, and fulfilled the requirements of both Lloyd's Register and the Board of Trade. I find from our surveyor's report that the sample from the particular plate which failed, which was 1¼ in. thick, stood a tensile strain of 29·6 tons per square inch, with an elongation of 20 per cent. in a length of 8 in., while strips cut from it were bent almost double cold. In fact, the material appeared, from the mechanical tests applied before it left the steel works, to be in every respect suitable for the purpose for which it was intended. One remark, however, may here be made, namely, that the plate in question was exceptionally large and heavy, viz., 20 ft. long, 5 ft. 6 in. wide, and 1¼ in. thick, weighing about 2 tons 16 cwt.

This material was built up into a boiler by a company who have had an unusually extensive experience in the manipulation of steel, having turned out no fewer than 175 boilers of this material. The plates were treated precisely as other steel plates have been treated in the same works, and with all the appliances which experience has shown to be necessary; all the holes were drilled, and the plate was then heated in a furnace and bent to the required curvature in a pair of powerful vertical rolls in the usual manner. Under the circumstances it appeared at first sight astounding to find the material tearing under a pressure which represents a strain of 6·7 tons per square inch only, or less than one-fourth of the strain which the original sample withstood. In addition to this, the appearance of the fracture indicates that the plate did not possess any ductility, stretch, or elongation whatever. Neither the steel makers nor the boiler maker have as yet afforded any satisfactory explanation of the occurrence. It is without doubt a most serious affair, especially in view of the high pressures which have now become so common.

* Institution of Naval Architects.

On hearing of this accident the committee of Lloyd's Register instructed me to investigate the matter, endeavor to ascertain the cause of the accident, and, if possible, recommend some measure to prevent such an occurrence in the future. My investigations were only completed last Tuesday, and as such a serious matter as this, which bears upon the safety of life and property at sea, must naturally give rise to no little speculation among engineers and steel makers, and has already produced great consternation in many quarters, I have taken this opportunity of laying before you a short statement of the facts as they have come before me, the results of my investigations, and the conclusion which I have arrived at, with a view of eliciting from the various steel makers and users here the benefit of their views and experience. Upon my visit to the boiler making works I was fortunate enough to find a sister boiler to the one which had burst, ready for testing. This boiler was tested in my presence to 300 lb. per square inch, and was carefully measured and gauged, and found to show no signs of deflection or yielding. I also ascertained from an examination of the testing appliances that an abnormal pressure could not possibly have been exerted at the time of the testing of the first boiler.

Seeing that the plates that broke had stood all the mechanical tests required before leaving the steel works, and that when worked into the form of a boiler shell it gave way at less than one-fourth of its original strength, it appeared at first sight that the plates had been in some way injured, or had undergone some material change from the time they left the steel works until they were riveted into the form of a boiler shell; therefore it became necessary to look carefully into the mode of manipulation of the plates in the boiler shop, and especially the heating and bending of them. One of the plates was bent in my presence. It was heated in an ordinary plate furnace, but when taken out was far from being of a uniform heat; the end of the plate near the door of the furnace was at a black heat, which gradually increased toward the other end to a dark red heat. Then the plate was turned end for end and again placed in the furnace, with a view to heating it so far as possible uniformly, but when again drawn out of the furnace it was seen that the heat was not at all uniform, one end being of a dark red or nearly black heat, which gradually cooled down to a blue heat at the other end. In this condition it was passed through a set of powerful vertical rolls, and bent to the required curvature.

The plate passed through these rolls six times, and by the time the operation was completed one end of the plate was quite cold, while the other end remained at a blue heat. It was thought that this unequal heating of the plate may have set up in the body of the plate excessive strains of a dangerous character, and that these strains were aggravated by rolling the plate at a dangerous heat, it being well known that the ductility of all steel becomes lessened when worked at a blue heat, and it is, I think, generally admitted that it is far safer to work steel cold or red hot than at any heat between these two points. Steel plates, and especially large ones, must be injured by such treatment, but as to the intensity of the strains set up, or their exact locality, nothing definite can be said. To ascertain the nature of the material as it stood, test pieces were cut from the fractured plate, both close to the fracture and apart from it, and subjected to tensile test at one of Lloyd's proving houses, with the following results, which the engineers have kindly communicated to me:

Samples.	Breadth.	Thickness.	Area.	Total tons.	Square inch, tons.	Extension in 8 in. per cent.	Extension in inches.	Contracted area.
	in.							
S. I. X.....	1	1 [illegible]	1·26	40·5	32·14	27·34	2 3/16	[illegible] × [illegible] & 1/32
S. C. H. I..	1	"	1·26	41·75	33·1	26·59	2 1/8	[illegible] × [illegible]
S. 2.........	1	"	1·26	41·5	32·93	21·27	1 11/16	1 1/32 × [illegible]
S. C. H. 2 X.	1	"	1·26	39·5	31·35	23·4	1 7/8	1 × [illegible]
S. XX......	1	"	1·26	37·5	29·7	21·8	1 3/4	[illegible] × 1
S. IXX.....	1	"	1·26	37·25	29·56	26·6	2 1/8	[illegible] × [illegible]
S. XXX....	1	"	1·26	38·5	30·5	28·1	2 1/4	[illegible] & 1/16 × [illegible]
S. I. XXX..	1	"	1·26	38·25	30·3	27·34	2 3/16	[illegible] × [illegible]

From these tests it appears that the proved tenacity of the plate ranges from 29·5 tons to 33·1 tons, while the elongation ranges from 21·8 per cent. to 28·1 per cent. in a length of 8 in. I may say that I corroborated these tests by others made from the same plate for my own information in London—the positions of these test pieces are shown on the diagram—and they were also corroborated by other tests made for the information of the steel makers. This range of about 4 tons in the tensile strength of a plate of homogeneous metal like mild steel is very unsatisfactory. I obtained samples of the plate, and submitted them to five eminent and independent metallurgists, who have kindly furnished me with the results of their chemical analyses, which are as follows:

Carbon.	Silicon.	Sulphur.	Phosp.	Manganese.
0·36	0·015	0·055	0·087	1·050
0·27	0·016	0·044	0·076	0·641
0·33	0·010	0·038	0·065	0·612
0·30	0·018	0·044	0·063	0·648
0·26	0·005	00·38	0·067	0·650

The most striking feature in these analyses is the large proportion of carbon shown to exist in the plate. It is particularly high for boiler plates. Material used for thin plates, say from ⅛ in. to ⅜ in. thick, to stand the same mechanical tests as these thick plates did, would not contain more than from 0·15 to 0·18 of carbon; and these facts led us to further experiments. In view of the great difference in the amount of carbon required in steel for a thick plate and a thin one to stand the same mechanical tests, it was deemed desirable to make an experiment which would determine to what extent work in the shape of rolling, and especially rolling thin plates, which during the latter part of the operation must of necessity be rolled, comparatively speaking, cold, affected the tenacity and ductility of the material. A slab of steel containing about the same amount of carbon as the plate that ruptured, viz., 0·33, was obtained at the steel works where the plate was made, and rolled at one heat down to ⅛ in. in thickness. This material, had it been rolled down to 1¼ in. plate, judging from the carbon it contained and the tests of the broken plate, as well as the opinion of the steel makers, would have had a tenacity of from 30 to 34 tons per square inch. It was found, however, that when rolled down to ⅛ in. thick its tenacity was increased to from 35 to 41 tons per square inch, with an elongation of from 21 to 24 per cent. in a length of 8 in. Other pieces were made hot, and quenched in water. These, when tested, broke at a tenacity of from 44 to 45 tons, and had, practically speaking, no stretch at all. Pieces were cut from the fractured edge of the plate, as shown on the diagram, and subjected to tensile, bending, and temper tests. They showed a tenacity of 33·5 to 34·2 tons per square inch, but they stretched only 13 and 16 per cent., and broke with a crystalline fracture, as will be seen by the specimens produced. They bent cold to a considerable degree, but when made red hot, and quenched with water, instead of bending, as pieces of a thin plate of similar tenacity and ductility would do, they broke under the first blow of a hammer without any bending whatever. The material was so high in carbon as to take a temper and become quite hard and brittle. Further cold bending tests were made from pieces of the broken plate, both before and after being annealed; those which were tested before annealing bent fairly well, strips ⅛ in. square bent to an angle of 49 to 61 deg., the fracture showing a considerable amount of alteration in form; while those pieces which were tested after annealing bent much better, in fact, almost double. Strips, however, that were heated and quenched in water broke short without any bend whatever at the first blow of a hammer, and thus corroborated the previous experiments made in London. These experiments point to the fact that the plate which gave way must have become partially tempered by the heating and cooling to which it was subjected for the purpose of rolling it into its cylindrical form. The heating not having been uniform, the tempering could not have been uniform, and the variations in the temper no doubt have caused the variations in the strength and ductility shown by the different parts of the plate. The hardest part of the plate yielding less than the rest became naturally more strained, and hence the plate tore at its hardest part at a pressure only a small fraction of that which it would have borne if its yielding had been uniform.

Having thus placed before you the nature of this accident, and the steps taken with the view of unraveling the supposed mystery, I now venture to state what inferences may, in my opinion, be drawn from the results of the investigation. I think it will be acknowledged that a material which is so high in carbon as to take a temper and break short as described, even if it possesses high qualities of tenacity and ductility before being tempered, must be looked upon as unreliable and altogether unsuitable for use in marine boilers. It would appear that the desire to obtain high steam pressures, and to use steel of a higher tenacity consistent with a large amount of ductility, has caused the marine engineering world to unknowingly drift into using a material of an unreliable and unsuitable character for the shells of marine boilers, more especially when the usage which such plates receive in heating and bending is considered; for except among steel makers it does not appear to have been generally known that the thicker a plate is the more brittle and erratic in its behavior it must become, as compared with a thin plate made to stand the same mechanical tests as far as tenacity and ductility are concerned, as, otherwise, I feel convinced that the increase in tenacity from 29 to 32 tons for thick boiler shells would not have been advocated. So far as I am concerned, and the Society which I represent, I may say that it has always been our endeavor to discourage the use of steel of high strength. The rules of Lloyd's Register require boiler plates to have a tensile strength of from 26 to 30 tons, and have done this from the commencement of the use of steel, because we felt that the higher the tenacity arrived at the more likelihood there would be of the plates giving trouble, and our whole desire has been to keep the material mild. We have, however, had considerable pressure brought upon us by manufacturers and engineers to allow a strength of 32 tons per square inch for thick boiler shell plates. This accident and the investigations which have followed clearly point out that engineers have been drifting toward the use of an unreliable material, or at all events a material which is too near the verge of danger to be pleasant, a state of things that should not exist with steam boilers. I would therefore urge, in order to remedy this growing evil, that the tenacity of steel plates for boiler shells—which are becoming thicker every day—should in no case exceed 30 tons; and that a temper test should be insisted on from every thick plate, and the practice of using enormously large plates should be discouraged; while more care should be exercised in uniformly heating and bending these plates. I have conferred with the principal steel makers in the kingdom on this subject, and am able to say that they agree with me, and are decidedly of opinion that steel plates over an inch in thickness, and having a tenacity of more than 30 tons, must contain so much carbon as to render them unsuitable for boiler-making purposes, although they may possess the necessary tenacity and ductility to withstand the usual tensile and cold-bending tests. I venture to hope that this paper will be made the subject of a discussion, with a view to obtaining further opinions respecting the important points in question.

AERIAL NAVIGATION.

DURING the last year a good deal has been said and written and something has been done in the matter of aerial navigation. A certain qualified success has been obtained in France under exceptionally favorable conditions by MM. Tissandier. On the 29th of September the Tissandier balloon made a trip near Paris which demonstrated that a balloon could be steered and propelled in a very high wind. The entire weight of the machine, including ballast and passengers, was about 1¼ tons. The balloon was shaped somewhat like a cigar, 90 ft. long and 30 ft. in diameter. It was inflated with hydrogen. A 10 ft. screw was caused to rotate by a Siemens motor, which was put in action by large bichromate batteries, four in number, each having six cells, with eleven carbons and ten zincs. Each cell held about six gallons of bichromate solution. The screw made 180 revolutions per minute, the motor making 1,800. A large rudder played an important part in maneuvering the machine. Too much has, we think, been made of these experiments. History repeats itself in ballooning as in other things, and the employment of electricity as a motive power shows how closely men have followed a beaten track in this department of mechanics. In early ages, when men knew naught of any power but that of animals, flight was to be effected by means of our muscular strength. No sooner had the steam engine become a practical machine, than it was enlisted by the aeronaut.

The storage battery has scarcely become a marketable commodity, and flight is to be accomplished by its means. If some new agent or motive power were to be introduced to-morrow, it also would be resorted to by those who seek to sail in the air. It is time, we think, that all such projects should be looked at from a common sense point of view; that is to say, from the point of view of sensible, intelligent engineers; and this is the more necessary because we believe, first, that the navigation of the air may not after all be impossible—who, indeed, can say what is and is not impossible in physical science?—and, secondly, because it may be easily shown that no sufficient success can be achieved by the aid of balloons and screw propellers, electrical engines and such like. As this is simply a question of facts and figures, it will not be difficult to make our meaning quite plain.

It may be taken that, in round numbers, 13 cubic feet of air weigh 1 lb. when the pressure is 14·7 lb. on the square inch. The weight of a cubic foot of air is 0·0761 lb. A silk balloon, 100 ft. long and 30 ft. in diameter, with conical ends, would have a capacity of about 60,000 cubic feet; less if the balloon were cigar shaped. Now 60,000 cubic feet of air weigh $\frac{60,000}{13}$=4,615 lb. One pound of ordinary illuminating gas has a volume of 30 cubic feet; consequently, our balloon filled with coal gas would weigh $\frac{60,000}{30}$=2,000 lb., and 4,615−2,000=2,615 lb., and this represents the buoyancy of the balloon; from this is to be deducted the weight of the silk, the net, etc. It will be seen that the margin left for passengers and machinery is small. Let us suppose that the balloon is increased to the enormous dimensions of 200 ft. long and 60 ft. in diameter. Then its capacity would be increased eight-fold, and it would lift 2,615×8=20,920 lb., or, say, a little over 9 tons. Deducting the weight of silk, cord, car, etc., it may, perhaps, be assumed that we should have left five tons available for passengers and motive power. If we deduct a ton for passengers, we have four left for machinery. We have next to see what this machinery would have to do.

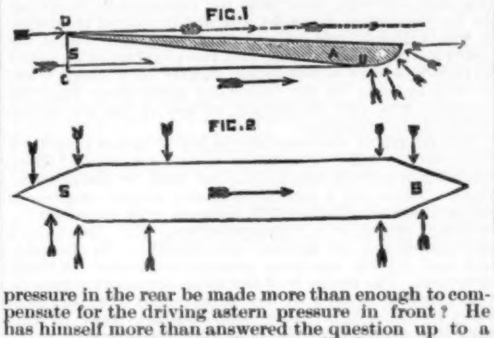
It will be at once conceded that for ordinary purposes of locomotion nothing can be better than a railway train unless it be faster, and one of the favorite arguments used by those who advocate aerial locomotion is that we should be able to move by its aid with much greater celerity from place to place than we do now. If, on the contrary, we are told that great speed is not aimed at, then the navigable balloon can be only of very limited utility. It would be of service in time of war, and might be used for purposes of exploration—like that in Jules Verne's celebrated story, which, however, was not navigable; but if aerial navigation is to be confined to such purposes as this, it would rarely be worth having at any price that could possibly be paid for it. Let us assume, therefore, that a speed of at least fifty miles an hour is demanded, by which the Atlantic could be crossed in seventy hours, or, say, three days, and Australia would be brought within ten days of our shores. If our navigable balloon moved in still air, it might encounter a resistance at fifty miles an hour of about 12 lb. per square foot of cross section. It may be argued that by having pointed bows this force would be diminished. We may concede this, but it must be borne in mind that the surface frictional resistance would be enormous. It has been shown, indeed, that in the case of railway trains it is their bulk, not the area of end advancing through the atmosphere, that measures their resistance. We are, perhaps, not far from the mark, we believe, when we estimate the resistance of such a balloon as that of which we are speaking, namely, 200 ft. long by 60 ft. in diameter, at 28,000 lb. A speed of fifty miles an hour is equal to 4,400 ft. per minute, and $\frac{28,000\times4,400}{33,000}$=4,036 indicated horse power. We can see no way of escape from this conclusion. It may be pointed out that a large ship can be driven across the Atlantic at twenty miles an hour with this much power, but it must not be forgotten that the resistance both in air and water increases in an enormous ratio with the speed. If we were content to drive our balloon at twenty miles an hour, the resistance would not exceed two lb. per foot, and the power required would be about one-fifth of 4,000 horse power, or, say, 800 indicated horse power. Again, a volume of water of 60,000 cub. ft.—that of our balloon—would weigh no less than 13,368 tons, and a ship with this displacement would require over 20,000 horse power to propel her at twenty miles an hour.

It requires no very brilliant engineering attainments to demonstrate that it is simply impossible to get 800 indicated horse power, or anything like it, out of a weight of machinery of only four or five tons. The thing is a physical impossibility. It appears, therefore, that the most that can be done consists in providing mechanism by which the course of a balloon may be modified. Thus, if the wind were blowing due east, then it is, we believe, quite possible to make a balloon pursue a course a few points to the north or south of east. It is also possible to produce motion at a very moderate velocity in a calm. Thus at five miles an hour the resistance to motion would be quite insignificant. We have made no allusion of any kind to the effects of storms, although they would have to be reckoned with. If it is proved to be impossible to take up power enough to propel a balloon in a dead calm faster than an ocean steamer, then navigable balloons can be of no practical utility whatever, save, as we have already said, for exploring purposes, when time is of no value in one sense. On the other hand, it must not be forgotten that the longer the time during which the machine is in motion, the greater the store of fuel, or its equivalent, that must be carried, other things being equal, and for this reason a machine which would only

move, say, at ten miles an hour might be quite useless for exploring purposes. For example, unless a navigable balloon could carry power enough to last, say, six hours, it could not be employed to survey an enemy's country to any extent. It may be taken as demonstrable that it is impossible to do anything in the way of general aerial navigation with balloons; are we then to assume that flight through the air is an impossibility for man now, and must always be so? Our reply is in the negative. At all events, it is a patent fact that navigation of the air is accomplished by living creatures without muscular effort. If we can only ascertain how this is done, then flying may become possible to man. We refer, of course, to the soaring of birds on extended wings without effort of any kind. We are at present in much the same position as a man who sees from some distance another riding a bicycle; the rider we may suppose, furthermore, to speak in an unknown language, and consequently to be entirely unable to explain to the looker-on how the thing is done. If the albatross could explain by what knack it could float for hours together without flapping a pinion, or if the crane could explain how it circles its way heavenward without effort, flight would probably become an easy thing to man; but as we have no means of learning save direct observation and experiment, it would be well, we think, that experiment took a new departure, and that aeronauts broke ground in a novel direction. What this is we may now proceed to explain briefly.

We published some months ago letters from Mr. Lancaster, on the flight of birds, which letters were of the most suggestive character. Residing in Florida, he possessed, for reasons which he fully explained, admirable opportunities for studying the mode of flight of certain soaring birds, and he arrived at a conclusion, the accuracy of which admits of no dispute, viz., that these birds can fly in a steady breeze without flapping their wings in any way, and accordingly without muscular effort. If this proposition be accepted as true—and we hold it to be, as we have said, indisputable—then the next step is to ascertain to what law of pneumatics there flight is to be credited. Mr. Lancaster has already made some experiments with limited means and a fair measure of success. Recently he has written to us again on the subject. He informs us that he is now once more in a position to push on his investigations, but before doing so he desires to have some expression of opinion from scientific men as to the probable accuracy of his views on the subject. He has written a paper setting forth these views, which is, we regret, too long for our pages, but we can, we think, make his theory clear without occupying too much space, and this we propose to do here.

Let it be assumed that a steady breeze is blowing in the direction of the arrows in the accompanying diagram, Fig. 1. On this breeze let us suppose that a flat board, whose section is shown by A, is supported. The effort of the breeze to carry this board along with it will be measured by the friction between the air and the board, and by the vertical area of resistance shown by the line, S, which is the sine of the angle B C D. The angular position of the board will deflect the breeze downward, and cause a reaction or pressure underneath, tending to lift the board up. The breeze above the board will be intercepted, and there will be a negative or reduced pressure on the upper surface throughout its whole region, as denoted by the dotted arrows. The air at the end, B, of the board will tend to rise, and rush upward into the partial vacuum, and in doing so will urge the board forward, as shown by the small radial arrows. That the board can be carried in the air this way is proved by the common paper kite. If, now, the pressure of the breeze at B—that is to say, at the round stern of the board—was greater than the combined pressure and friction of the breeze driving the board astern, then the board would advance against the breeze, and sail so long as it kept its balance, just like the kites, cranes, or albatrosses, and the problem of flight would be solved. Provided man could learn to keep a flat plane beneath him balanced, as a bird does, he too could fly against a steady wind. The question Mr. Lancaster asks is simply, Can the



pressure in the rear be made more than enough to compensate for the driving astern pressure in front? He has himself more than answered the question up to a certain point, because he has actually, he states, succeeded in making plane surfaces fly unaided against the wind for considerable distances; but the question may be considered from a theoretical as well as a practical point of view.

The late Mr. Froude showed that the whole resistance offered by a homogeneous fluid to a body moving through it consisted in skin friction and the production of eddies. In other words, the water closing in round the stern of a ship tends to drive her forward precisely as much as the driving of the water outward by the bows tends to keep her from going ahead. Thus, then, the two forces balance each other. Let us suppose that Fig. 2 represents the bows and stern of a ship, right lines being used instead of curves, that the angular action may be clearer. Here it is evident that the retarding influence of the outstreaming water, as shown by the arrows, at the bows, B, is compensated for by the accelerating influence of the in-streaming water at the stern, as shown by the arrows at S. The flat sides of the craft take no part in the conflict. The resistance to be overcome is simply that due to friction along their straight sides. Now, if a similar proposition holds good of an elastic fluid, such as air, it will be seen that the driving astern pressure or effort due to the sine, S, Fig. 1, will be compensated for by the action of the stern, and equilibrium will result. The float we have sketched would, so long as it retained its balance, remain in one place. We have, however, the friction yet to account for, and it is clear that the stern action must be sufficient to compensate for this, as well as balance the stress on the bows, so to speak, or there can be no advance. Is it possible so to construct a body that the required result will be obtained? So far as can be seen, birds are so constructed, and we may add that Mr. Lancaster's own experiments tend to prove conclusively that the problem can be solved.

An investigation of this problem can, we think, hardly fail to prove interesting to our mathematical readers, and to them we commend it, contenting ourselves with repeating that Mr. Lancaster's propositions appear to be worthy of the most careful and attentive consideration by those who do not believe that we already know all that can be known about everything. The enunciation of Mr. Froude's proposition created a great deal of surprise, because it was a wholly unexpected result of experiment and mathematical investigation. It was, in a word, a discovery. It is not at all impossible that the world is on the brink of a cognate discovery concerning aerial navigation.—*The Engineer.*

CONSTRUCTION OF THE WHARF WALLS OF THE NEW FLOATING DOCK AT HAVRE.

Peculiar Conditions under which the Work had to be Effected.—The west basin of the new dock at Havre was to be 1,440 feet in length by 720 in width, and the wharves to be 4,135 feet in extent. Two-thirds of the latter had to be constructed in the mouth of the Eure upon ground covered with from 6 to 15 feet of water at high tide. As they had to be founded 3·28 feet beneath the future bottom of the basin, the opening of a trench nearly 25 feet in depth by ordinary methods could not be thought of. Such a trench, which would have been filled with sea water twice every twenty-four hours, would certainly not have stood, and it would have been almost impossible to pump it out in the space of time comprised between the running out and in of the tide. For this reason it was decided to employ masonry wells in the construction of the walls, as had been done previously at Bordeaux and Saint Nazaire. The homogeneous nature of the earth, moreover, which is formed almost entirely of a stratum of compact clay, justified the use of this process.

Dimensions of the Walls.—The walls were constructed of a series of blocks, 32·8 feet long by 22 wide and 26·25 high, containing a well in the center, which widened out at the bottom. A first series of wells was in the first place constructed, and a space left between each block for a second. The intermediate wells were not undertaken until the first had reached the proper depth. The object of this precaution was to prevent the sinking of one block having any effect on its neighbor. The space reserved between the contiguous blocks was uniformly 3·28 feet.

The Order followed in the Construction.—The sinking of the wells was regularly conducted as follows: The masonry, which was formed of rubble cemented with a mortar made of 880 pounds of Portland cement to 35 cubic feet of sand, was first built to a height of 14·75 feet. Thirty days after this the earth was removed from the interior, so as to sink the block entirely in the ground. This first operation finished, the masonry was built to its definite height and then left to itself for twenty days, in order to harden. Then the

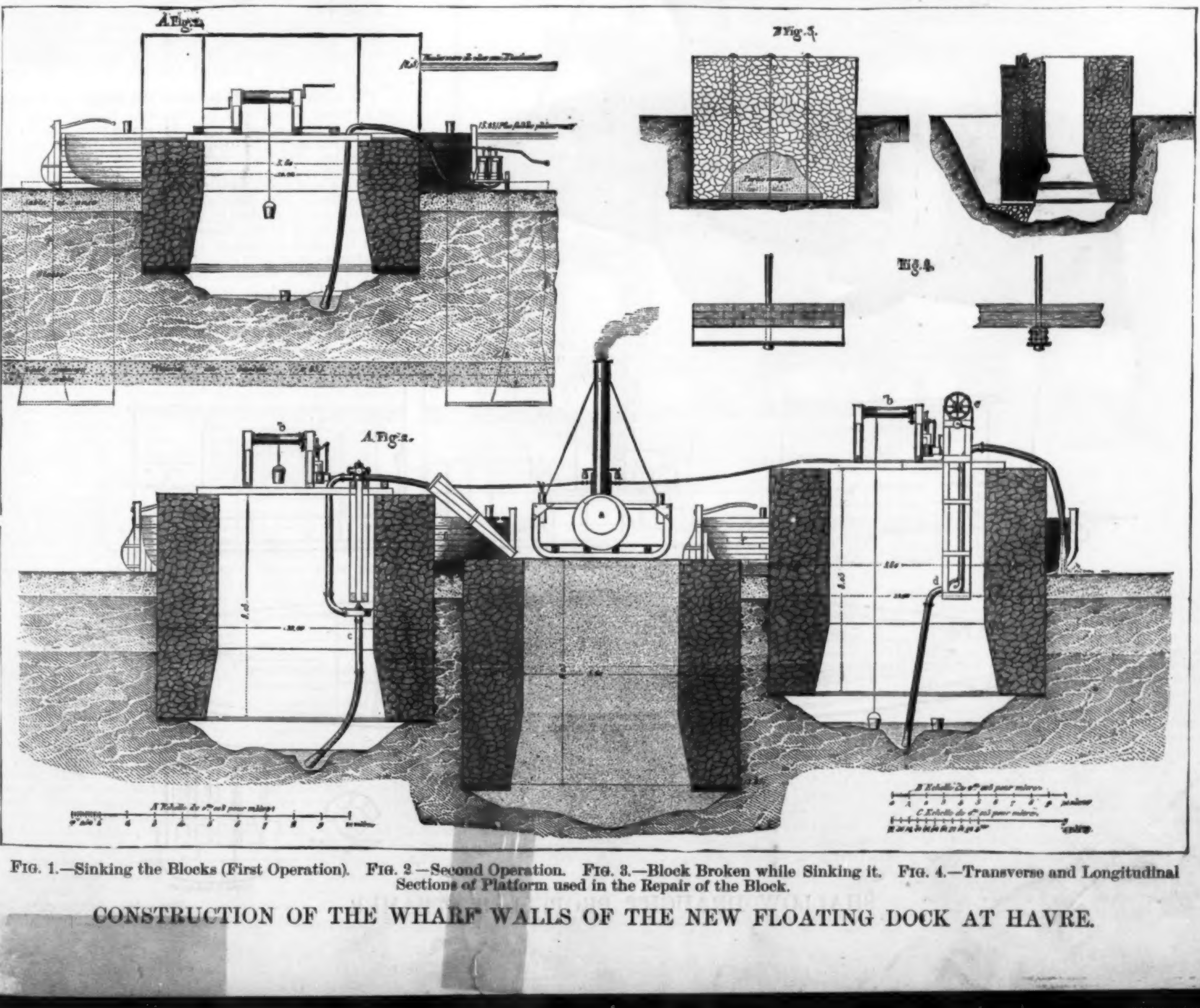


FIG. 1.—Sinking the Blocks (First Operation). FIG. 2.—Second Operation. FIG. 3.—Block Broken while Sinking it. FIG. 4.—Transverse and Longitudinal Sections of Platform used in the Repair of the Block.

CONSTRUCTION OF THE WHARF WALLS OF THE NEW FLOATING DOCK AT HAVRE.

operation of sinking was begun again, and continued until the bottom of the block reached the level selected for the foundation. Finally, the well was filled with beton. The number of wells constructed in this way was 84, representing a total amount of masonry of 1,589,400 cubic feet. The construction of the first well was begun April 16, 1881, and the last was finished March 20, 1884.

According to the project, the blocks were to have been established upon frames formed of three courses of beech planks, having in plan the same dimensions as the masonry; but, as experience had shown at Saint Nazaire that blocks without frame behaved exactly like those with one, it was decided to lay the masonry upon simple planks.

Out of the 84 blocks, only one broke during the course of sinking it, and the accident in this case arose from the block not having been allowed to harden long enough. It became necessary to make an excavation in front of the block, destroy the portion that had become detached, and to reconstruct that part (Fig. 3).

Pumping Out the Water.—The pumping of the water from the wells was under the charge of the contractors. During the first period of sinking, the apparatus employed were simply Letestu pumps worked by manual power—eight men to a pump (Fig. 1). Thanks to the impermeable nature of the ground, when once the well was empty it remained so during the entire duration of low tide. But the same process was not applicable to the second period of sinking, not only on account of the time it took, but also to the height of the suction. The contractor therefore decided to have recourse to small centrifugal pumps, with vertical axle, actuated by a three-cylinder Brotherhood engine. The entire affair was mounted upon a wooden framework in the interior of the well (Fig. 2). Steam was furnished by a generator placed upon a flatboat moored alongside of the block.

Removal of the Excavated Material.—The excavating was at all times effected exclusively by manual labor. The number of men employed at a time was usually four. They began by making an excavation in a vertical direction, and finally extending it under the block. No particular precaution was taken to secure an exactly plumb sinking of the blocks. When they inclined to one side, the workmen were shifted to the opposite side. The excavated earth was shoveled directly into a wooden bucket, which was at first hauled up by a wooden windlass affixed to the top of the block. These windlasses were eventually replaced by steam ones. The earth was emptied into punts, which dumped it at high water at a minimum distance of 110 feet.

In short, this process of building foundations by means of masonry wells sunk in open air has been a complete success in the construction of the new floating dock at Havre. This success is due to the proper proportion observed between the length and width of the blocks, to the use of Portland cement for constructing the masonry, and to the strict observance of the rule to allow a certain amount of time for the mortar to set. The mode of sinking employed was more economical than the use of compressed air would have been.

DESCRIPTION OF FIGURES.—*a*, steam generator; *b*, steam windlass; *c*, centrifugal pump; *d*, direct-acting centrifugal pump; *e*, hand windlass; *f*, punts.

LIGHT DRAUGHT FAST SCREW LAUNCH.

THE boat in question was built last year by Messrs. Thornycroft & Co. to the order of Messrs. Allen, Anderson & Co., of Alexandria, for the use of the directors of the Societe d'Irrigation dans le Behera, and was intended principally for running on the Nile from the nearest railway station to the irrigation works, a distance of forty-five miles. The problem was how to do the journey, including the railway traveling, in one day, allowing time also for transacting business. As the maximum draught was not to exceed 18 inches, it was evident that an ordinary propeller would require too great a draught, and the purchasers objected to paddle-wheels as not being sightly on a small boat. It will be seen by the following particulars that the question was very effectually solved by the use of the guide blade propeller.

The length of the boat was 56 feet 8 inches long by 7 feet 8 inches wide. She has a cabin forward, and there is a long open well aft, as may be seen by the illustrations. There are a pair of ordinary simple engines of the well-known Thornycroft pattern, with cylinders 6¼ inches in diameter by 8 inches stroke. The slide valves are balanced in the manner introduced by Mr. Donaldson, and which has been already illustrated in these columns. The boiler is of the usual locomotive marine type, such as is used in the second-class torpedo boats built by this firm. The following are some of the principal elements of the boat and machinery:

	Ft.	In.
Length over all	56	8
Beam	7	8
Draught of water on trial, forward	1	1⅛
" " " aft	1	3
Displacement	6·7 tons.	
Height of tip of blade of propeller above normal water line with boat at rest on trial draught	5¾ inches.	

	Cwt.	Qr.	Lb.
Weights on board at trial (coal)	3	2	0
Fourteen men	20	0	0
Total	23	2	0

Engines:

Diameter of cylinders	6¼ inches.
Stroke	8 "
Number of main bearings	3
Length and diameter of journals	4½ in. by 2½ in.
Length and diameter of crank-pin journals	3½ in. by 2¼ in.
Material of bearings	gun metal with white metal liners.
Point of cut-off with link in full gear	0·72 of stroke.
Diameter of propeller	20 inches.

Boiler:

Barrel of steel, fire-box and tubes of Lowmoor iron.	
Grate area	6·7 sq. feet.
Number of tubes	170
" stay tubes	6
Diameter of " "	1¼ inches.
Heating surface: Fire-box	18·5 sq. feet.
Tubes	225·5 "
Total	244·0 "

The following figures show the distribution of weights according to the original estimates. These worked out accurately within a trifling difference:

	Tons.
Weight of hull	1·59
" fittings	0·58
" engines	0·72
" boiler and water	2·80
	5·69

The following is a copy of the official report of the trial trip:

No. of Runs.	Pressure in Boiler.	Revolutions of Engines. Per Half Mile.	Revolutions of Engines. Per Minute.	Observed time.	Speed due to Time.	First mean speeds. Statute miles.	Second mean speeds. Statute miles.
	lb.			min. sec.			
1	134	938	527	1 49	16·514		
						18·202	
2	134	797	528	1 30½	19·890		18·339
						18·476	
3	135	929	527	1 45¾	17·062		18·559
						18·643	
4	135	789	530	1 29	20·224		
..	....	868	528	True mean speed.		....	18·449

Engines in full gear and stop valve full open.

The mean indicated horse-power developed was 102 on above trial.

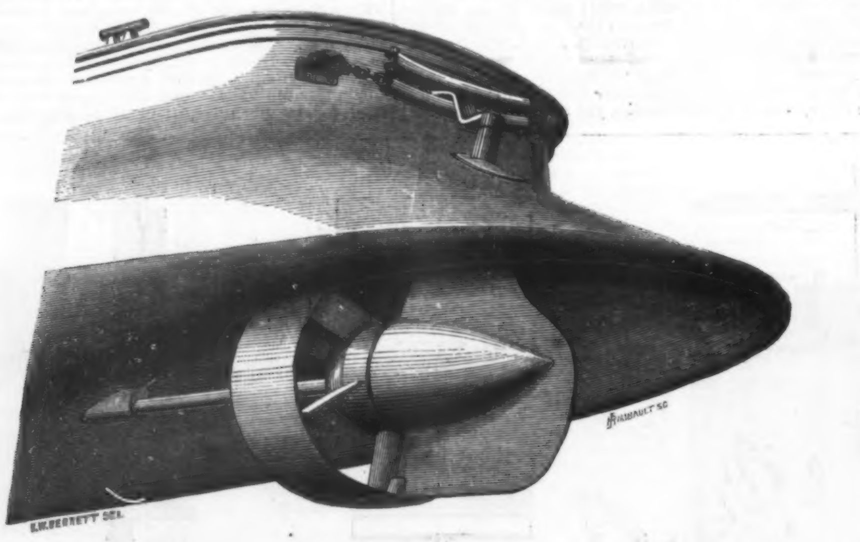
In the illustrations herewith, Fig. 1 is a profile, Fig. 2 a plan, Fig. 3 a section through the cabin, and Fig. 4 a section just abaft the propeller. In the latter view the shape of the prolongation of the boat aft is shown, together with the casing inclosing the screw. The engraving, Fig. 5, is taken from a photograph, and shows the general arrangements of the guide blade propeller.—*Engineering.*

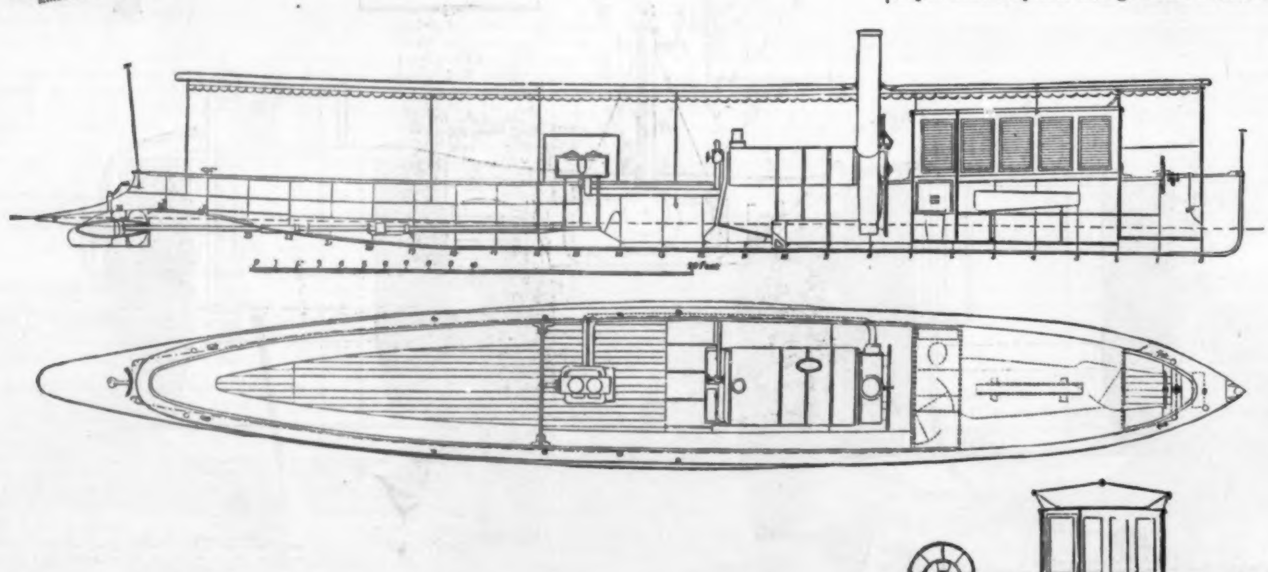
OLIVIER'S MATHEMATICAL REEL.

ABOUT a year ago the Societé d'Encouragement awarded one of its highest prizes to an apparatus invented by Mr. Louis Olivier for reeling threads of all kinds and numbering them with accuracy. After constructing various forms of the apparatus, the inventor definitely adopted the one which we figure on next page.

The ratio that exists between the length and weight of a thread constitutes its number. In spinning mills the two terms of such ratio are given by a special machine that makes known the length of the thread and scales that give the weight of the length measured.

The ordinary apparatus designed for this work consists of a reel upon which the thread winds. If the reel be a cylinder, the distance passed over by any point of its surface is evidently obtained by multiplying the circumference of the base by the number of revolutions that it makes around its axis. Up to the present, manufacturers have been content to accept this as the length of the thread wound. Thus an evident error is committed. In fact, in measure as the reel revolves, the thread in winding upon it becomes superposed, and this has the same effect as if the diameter of the reel kept constantly increasing. The extent of such error





SHALLOW DRAUGHT PROPELLER STEAMER.

is a function of two independent variables—the diameter of the thread and the length reeled. The same fabric is often made up of threads whose diameters are as 1 to 40. Extremely unequal lengths are therefore wound, and yet the reel that winds them one after another makes in all cases the same number of revolutions, and indicates the same measurement for all.

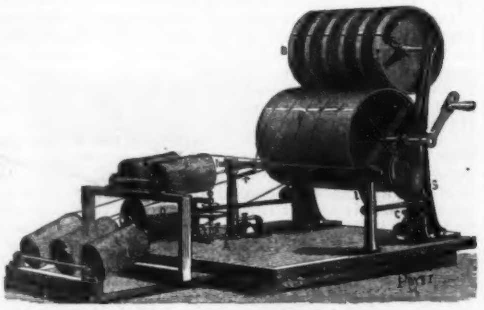
On another hand, as the error increases with the number of revolutions of the reel, it becomes necessary to operate with short lengths only, and consequently to repeat the operation several times in order to sample the same thread.

Thus, manufacturers, with the present processes, must content themselves with an extremely variable approximation in the measurements indicated by their reels. The exact determination of the ratio between the length and weight of threads is a very important matter; first, because in properly made fabrics it is necessary that the diameter of similar threads shall be exactly the same, and second, because spinners are paid, not at pound rates, but according to the length spun. Such length is too great to allow it to be measured directly, and it is therefore calculated according to the weight and number of the thread delivered. So it becomes the object of incessant quarrels between the buyer and seller.

Mr. Olivier's reel does away with the cause of the error that we have just spoken of. The apparatus consists essentially of a large drum, A, and of a small reel, B, both of metal, placed horizontally, and so superposed that the axle of B is a little behind that of A. Owing to this arrangement, the threads pass over the surface of the drum, A, without slipping, and wind without abnormal traction upon the reel, B. The drum communicates its motion to the reel by traction. The axle of the reel, B, is capable of rising vertically between the two prongs of a fork in measure as the thread passes over its surface. The velocity of its rotation is retarded proportionally to the thickness of the thread wound, while the revolution of A undergoes no variation. The length of the thread reeled is therefore equal, inch for inch, to the space passed over by any point whatever of the surface of the drum, A.

At every hundred yards of thread wound, the revolution counter, C, gives a signal. Besides, a special register inscribes the number of revolutions of the drum, A.

The counter carries an eccentric that gives the thread a to and fro motion in order to distribute it better on the reel. The latter is provided with a movable sector, *b*, which is pushed into the interior in order that the finished skeins may be removed more easily.



OLIVIER'S MATHEMATICAL REEL.

The error due to the superposition of the thread is not the only one that the inventor has desired to suppress. In other apparatus of this nature there is another one, which, though less important, is worth noting. According as the operator revolves the reel with more or less speed, the thread is more or less stretched, and this results in differences in the measurement of the lengths. The choice of the tension at which the thread is to be kept during the operation is entirely arbitrary. It belongs to chambers of commerce to decide this, not to a private individual. In the present state of things, what is important is that, whatever be the tension selected, the reel shall permit of its being kept constant. Such a result has been obtained by Mr. Olivier as follows: As the tension is a function of the velocity of the reel's revolution, he has endeavored to render the velocity uniform. To effect this, two signals are given the operator as soon as it tends to exceed that which is adopted as normal, one of them addressing itself to his sight and the other to his ear. The first consists in the uniform coloration of a drum, D, whose surface remains, on the contrary, divided into alternate red and green bands when the normal tension is not exceeded, and the second is a bell which is struck by small metallic pendulums, E, as soon as the tension increases.

The figure shows the arrangement of the bobbins, *k*, *k*, *k*, for carded wool. The position of the supports is modified according to the nature and state of the products and the form of the skeins or bobbins. These accessory pieces, however, belong to the old reels also. What especially distinguishes the apparatus under consideration is less the details as a whole than the fundamental principle of construction.—*Le Genie Civil.*

IMPROVEMENT IN LOOMS.

THE system of weaving with the open shed seems, as it deserves, to become more popular with manufacturers, and several loom makers who some time ago did not make dobbies that worked on this principle now manufacture them. It was a most important step in advance when the Keighley firm of Hattersley and Son introduced a dobby that answered the requirements of the case. The adoption of this machine was very extensive, and it came to be known as the Keighley dobby. The advantages of the apparatus may be said to be as follows as compared with the system of shedding from the center: The movement of the warp and healds is much less, because only that section or sections of the warp is every pick moved from the top to bottom, or from bottom to top, as is desired to make the pattern, all the sections of the warp remaining stationary at the top or bottom of the shed. The durability of the healds on account of the lessened movement and friction was considerably increased. Further, with the older system the weft is beaten up when the warp is closed, whereas with the open shed the weft is beaten into the angle made at the fell of the cloth by all those top and bottom shafts of warp that at the time may not be moving to change the pattern for the next pick. It is contended by the advocates of weaving on this system that the blow required to drive the weft home must be much less by beating it in the angle made when the warp is separated than to drive it home in the closed shed. They claim, therefore, that by working on the open shed principle tenderer warps may be dealt with.

The machine, which we now illustrate in Fig. 1, is an improvement on the Keighley dobby, of the type in which the shafts are pulled down with weights or springs. The elements of the motion are illustrated in Fig. 2, and any one, by comparing with a Keighley dobby, can see in what the improvement consists. This invention, and also the under motion, we should say, is due to Mr. Catlow, of the firm of Messrs. Butterworth and Dickenson, loom makers, Burnley. As will be seen from Fig. 2, each shaft has two of the parts *d d* to operate it, which is done by lifting them so that the knives, *e e*, which rock backward and forward, can catch the projections made on the upper edges. In the Keighley dobby these parts *d d* are governed by a single cylinder, and, therefore each lattice bar has to have two sets of pegs,

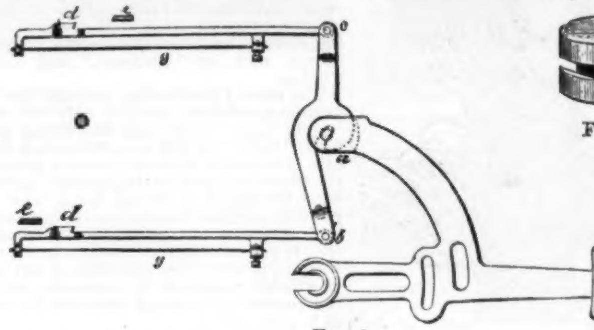


FIG. 2.

FIG. 3.

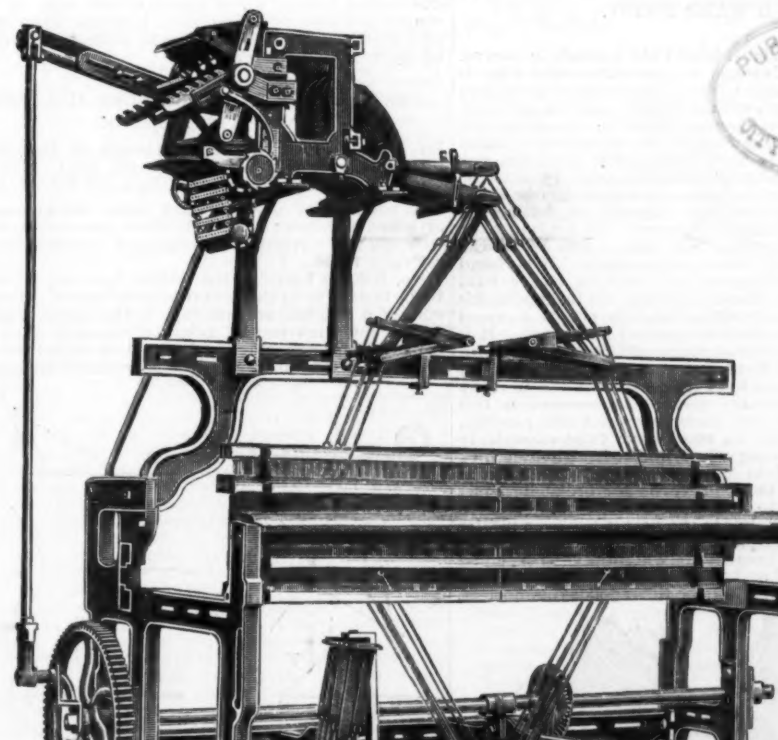


FIG. 1.

IMPROVEMENT IN OPEN-SHED LOOMS.

one to operate the top, and the other the bottom of the parts, and the arrangement also included needles and heavy ended levers. This fact of there being two pegs for one shaft was considered a drawback, the pegs had to be made rather fine, and had to be placed very accurately in the lay. They were so fine that we understand their breakage was a tolerably frequent accident. In Mr. Catlow's arrangement, however, as will be seen in the perspective view, he adopts two pattern cylinders, which completely overcomes the difficulty, and also allows him to dispense with the needles and heavy ended levers. By using two cylinders, one to act on the top, and the other on the bottom one of the parts, *d d*, the same number of pegs are required; but being in two lattice bars, they may be placed at twice the pitch as when only one cylinder is used. They may, therefore, be made considerably larger and stronger, and the certainty of their action is increased very greatly. In the arrangement under notice the pegs act on the slight steel springs, *g*, which, while it lifts the part into contact with the drawbar or knife, admits of every deflection.

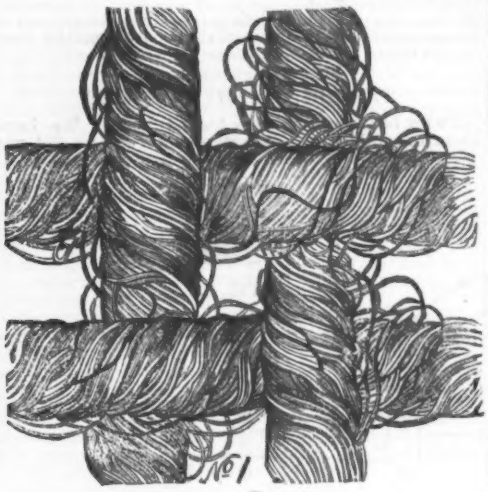
A part of the improvement relates to the mode of making the main joint at *a*, which, as usually made with a small plain rivet, is, we understand, a great source of trouble. We illustrate the pin or stud used in Fig. 3. As will be seen, it is large and substantial, and provides ample wearing surface. It is placed in position first of all in the jaw at joint, *a*, and the lever is then placed in the jaw with its part on the two nicks shown on the pin, a method at once simple, secure, and effective.

The under motion is a useful arrangement for weaving strong and heavy goods. The dobby is not a positive one on the down stroke, the shafts being pulled down by springs. With heavy sections of warp these springs are scarcely sufficiently strong. With the under motion however this defect is made up perfectly; the springs pull the shafts down a little below the center, when the lever of the under motion acts upon the ends of the bottom jack levers, and presses them firmly downward, the shed being then virtually the same as if made with a positive dobby. This motion has been found very useful for heavy weaving; if, however, the loom is engaged on light goods, when the tension of the springs is sufficient to form the shed, the motion may be put out of gear.—*Tex. Manufacturer.*

THE PHILOSOPHY OF TWIST.

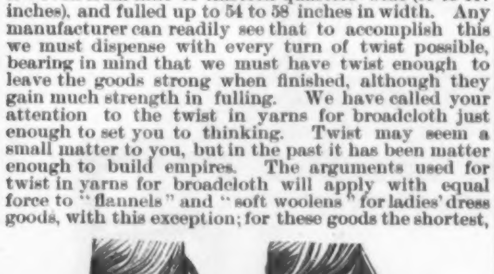
TWIST has been a mystery for all time. We have known its value and use, but what it was we did not know. Philosophers have given it attention, and manufacturers have tried to explain it, and yet after all the question remains, "What is twist?" We believe it was Evan Leigh that suggested that it was an application of the screw principle. In this he was certainly correct, but he failed to grasp the whole problem. Twist is yet the only practical method known of compressing fibers together so they will remain, and produce sufficient strength to weave and resist the friction and the wear and tear of manufacturing. It seems to us that twist is an application of the screw principle, which, as applied in mechanics, produces an outward pressure. It is the same when used as a propeller. The pressure is outward, hence the propulsion of the vessel. The screw principle applied to form twist in yarn is just the reverse, and compresses, no matter whether it is applied to the fibers of nature or those produced by man from metals or other substances. The sharper the screw (twist), the greater the pressure inward, and the smaller the thread or rope and the greater the strength, that is, to the extent of a natural twist. We make no woolen yarn without twist, for the simple reason that it is impossible. We can make cloth without yarn, but

we cannot make yarn without twist. We could not draw it, and if drawn it would be simply wool drawn out, without strength to weave. Then the fact of the matter is, we put in twist to enable us to draw the fiber into a thread, and additional twist to gain strength in the yarn and to produce effect. Felted goods are made without yarn, but they are unsatisfactory except as carpets or for loose garments. Many devices have been invented and used to overcome the defect in plain felts, but all devices for this purpose have proved unsatisfactory, so far as men's clothing is concerned. Let us look over the field, and see if we cannot find out something about the twist in yarn and why it is put there. We will commence with broadcloth, as it is the plainest cloth made, and answers our purpose best at this time. The object in putting twist into yarn for broadcloth is, first, to enable us to make the yarn, and if the stock is good, and has been carefully manipulated, and good oil used, we can then draw a good thread with very



ALL WARP TWIST.

little twist. But it must have twist enough to weave. When we get to this stage, we cannot consider what is best for the cloth alone; here the question of profit and production interferes with our plans, and we must put in a little more twist than our better judgment would dictate; but when these goods are woven on a hand loom, as many of them are to-day, we can have our own way a good deal as regards the quantity of twist; and under these circumstances we will make our filling with very little twist, and depend on felting for strength. As each bobbin is woven wet we must be very careful, or using soft twist filling will cause the loss of a good deal of time by breakage. If a hand loom weaver is paid by the day, and he is a careful man, he can weave almost anything, for he handles his picking gear with a sensitive hand to propel a wheel shuttle, and can regulate his stroke to his stock. It is then merely a question of wages with him. Broadcloth is woven from nine to thirteen quarters wide (99 to 117 inches), and fulled up to 54 to 58 inches in width. Any manufacturer can readily see that to accomplish this we must dispense with every turn of twist possible, bearing in mind that we must have twist enough to leave the goods strong when finished, although they gain much strength in fulling. We have called your attention to the twist in yarns for broadcloth just enough to set you to thinking. Twist may seem a small matter to you, but in the past it has been matter enough to build empires. The arguments used for twist in yarns for broadcloth will apply with equal force to "flannels" and "soft woolens" for ladies' dress goods, with this exception; for these goods the shortest,



WARP AND FILLING TWIST.

cheapest stock is often used and we gain little or nothing by fulling; consequently, we must look out for this in the stock and twist of the yarn, for these goods are made with the twist and not in the mills, as are English broadcloths.

When we come to suitings and trouserings, there is where the scientific skill of the designer will most avail him, and he will find it all important, especially in duplicating or imitating styles made by others. In such cases the twist in the yarn to be imitated should be carefully counted—that is, the number of turns per inch—for a variation of but one or two turns per inch, all else being favorable, will prove fatal. When fancy twist for effect is used, effect is then the controlling motive. The twist used may not be best for the cloth as a fabric, but effect in this case is everything. There is no pattern made that the twist best adapted for it should not be carefully studied, as much so as the pattern itself.

We have argued, ever since we have studied twist and its effect, that cotton cloths, and even prints, would be much improved in appearance, would absorb more coloring matter, hence be more durable, and would feel and wear better, if made with warp and filling of reverse twist. This view is fully maintained by Mr. Robert Howard, who states that, "for fine goods, the filling is twisted contrary to the warp, occasionally, to obtain a better and more nappy feel to the cloth."

There is much in manufacturing, beyond the senses of sight and touch, that is overlooked by manufacturers generally, and one of these is twist and its effect in different classes of goods. The mind, aided by the microscope, will penetrate where the sight and feeling cannot, and will open up a new world to thinking man. To illustrate our meaning, we have had two engravings made, not imaginary ones, but from actual threads magnified. It would seem at first thought that warp and filling made from one twist would be the most natural one; but we forget that we do not use them side by side, but at right angles with each other, entirely changing their nature and relationship to each other, as will be seen by referring to illustration marked No. 1, where the threads cross each other at right angles, and the fibers also cross each other, but diagonally, which is the harshest possible contact. It is hard in every sense, hence it is the method we pursue when we make a threadbare fabric, such as "Birdseye," and some other similar weaves.

The second illustration, marked No. 2, is made from yarn magnified as in No. 1, only the warp is warp or "right hand" twist, and the filling is filling or "left hand" twist. It will be noticed that the twist in each case is precisely alike in quantity; hence at the crossing the fibers are parallel diagonally, and will bed together when the filling is driven home, and this is the best possible method to make any goods that are to be felted and have a cloth finish, such as broadcloths, kerseys, etc. It is a common error to put much more twist in the warp to procure strength than is put in the filling, all of which approaches, in a measure, the unnatural crossing produced by using one twist for warp and filling, as in plate No. 1.

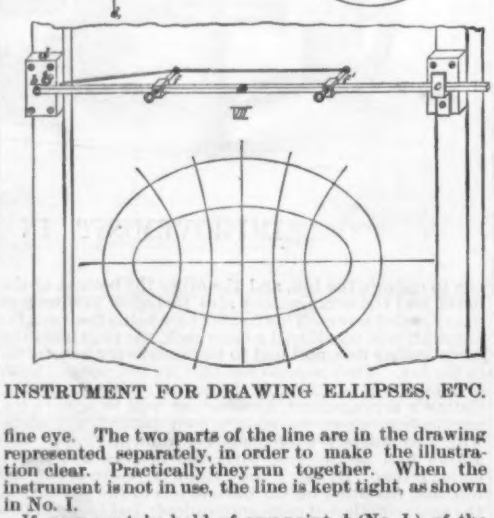
In alpacas, silk warp flannel, delaine dress goods, and similar fabrics, where cotton or silk warp is used simply as a base or foundation, it makes little difference about the twist in such goods, as the warp is covered up entirely.—*Wade's Fiber.*

INSTRUMENT FOR DRAWING ELLIPSES AND HYPERBOLAS.

By Frederic R. Honey, Instructor in Descriptive Geometry and Drawing, Scientific Dept. Yale College, Lecturer in Perspective, Yale Art School, etc.

A portion of a blackboard with the instrument attached is shown at No. VII. The principle of its action and the method of operating it are explained by referring to Nos. I.–VI.

No. I. a is a barrel fitted with a light spring which tends to revolve in the direction of the arrow. The two ends of a fine line are attached to the barrel, and the double line thus formed is wound round it. The loop is passed through the eye, f, and then round the eye, f^1. The two dots at f and f^1 represent a section of a



INSTRUMENT FOR DRAWING ELLIPSES, ETC.

fine eye. The two parts of the line are in the drawing represented separately, in order to make the illustration clear. Practically they run together. When the instrument is not in use, the line is kept tight, as shown in No. I.

If, now, we take hold of any point, b (No. I.), of the line and pull it, as the barrel is made to revolve the two parts of the line will be paid out equally, one part round the focus, f, and the other part round the focus, f^1. The locus of the point b (No. II.) will be one-half of one branch of a hyperbola. It will be seen that the focal distances R and r increase at the same rate, that is, $R-r=$ a constant length, viz., the transverse axis. The locus of the point b^1 (No. I.) is the other half of this branch, shown in No. III. If we measure a distance $f\,c=f^1\,b$ (No. I.), and pull on the line at the points c and c^1, the locus of these points (No. IV.) will be the other branch of the hyperbola. If we bisect the distance $f\,f^1$ (No. I.), at the point d, and pull on the line at d and d^1, the locus (No. V.) will be a straight line on which may be laid off the conjugate axis of the curve.

To draw an ellipse (No. VI.), extend the line so that the point d is at the vertex of the conjugate axis. If the scribing point be made to slide on the line (the barrel being prevented from revolving), the locus is a semi-ellipse, for the sum of the focal distances is constant. Similarly, the other half of the curve may be drawn.

In No. VII. the barrel is incased in a shallow box, d, which is screwed to the frame at the blackboard or made adjustable. A light steel bar, a, is hinged at b, and when in use is supported at the other end by a hook, c. The saddles, f and f^1, which carry the eyes through which the line passes, are adjustable to any point on the board, and may be set by means of the thumb screws at any distance apart. That is, the foci of the ellipses and hyperbolas are adjustable, and we can draw curves as long as the length of the line which is wound round the barrel will permit.

No. VII. represents cofocal ellipses and hyperbolas which are drawn with this instrument, all the hyperbolas being normal to all the ellipses.

In drawing ellipses a set screw, E, will hold the line at any desired point.

For the drawing board the saddles, f and f^1, are made to slide on the blade of a T-square, and the barrel is incased in the stock. A T-square is thus made available for drawing ellipses and hyperbolas.

The crayon for the blackboard or the pencil for the drawing board is provided with a hook attached close to the scribing point. With this hook we can take hold of the line for drawing hyperbolas, and it can be made to slide on the line in drawing ellipses.

THE TELEPHONIC TRANSMISSION OF SPEECH BY DISCONTINUOUS ELECTRIC CURRENTS.

By L. de Locht-Labye.*

An experimental analysis of the working of the hammer telephone has led me to make a profound study of the complex phenomena of the telephonic transmission of articulate sounds, and I have reached the conclusion that I formulated as follows in a preceding article: Speech, like all sonorous vibrations, is transmitted telephonically by a series of electric currents of very short duration, that succeed one another without continuity upon the line terminated by a receiver.

Despite the prestige exerted by the name of the celebrated Boston professor (Bell), and the credit attached to the theory of undulatory currents through the merit of its author, it does not appear possible to me that an enlightened and impartial reader can resist the mathematical precision of my reasonings and the rigorous accuracy of my deductions.

In this study I rely exclusively upon the known applications of electricity and upon the elementary principles of mechanics. Nothing, then, is subject to controversy, and nothing can be contradicted. Should my conclusions give rise to criticism or objection, I should be happy to have the latter formulated. It is without any other desire than that of getting at the truth that I have prosecuted this work, and I shall therefore gladly accept every means that may be proposed for attaining such object. What is the precise meaning of the theory of undulatory currents? This is the way I understand it: The emission of speech in front of the flexible disk of the Bell telephone brings about undulatory motions thereof. These motions are the *facsimile* of the sonorous waves of the air, that is, they have an equal duration and like phases, the amplitudes alone being different. These vibrations of the magnetic diaphragm in front of the poles of a magnet produce in the wire bobbins wound round the magnet in the vicinity of the poles *continuous* electric currents, the progressive variations in which present a character that is identical with the sonorous waves of the air, and of the inflections of the diaphragm which are, so to speak, the continuation of it.

For this reason the electric currents have been denominated *undulatory*, by Prof. Bell, in contradistinction to those that he calls impulsive and intermittent currents, the production of which is *sudden*, and which are *discontinuous*.

In the telephone receiver under the action of the undulatory currents, the flexible diaphragm undulates in absolute concordance with the diaphragm of the transmitter. The sonorous waves that result from this vibratory motion all agree, in every respect, intensity excepted, with the sonorous waves that were the origin of its telephonic transmission.

Prof. Bell's theory may be enunciated, then: A persistence of the undulatory motion, without any alteration in the duration or form, in all the successive phases of the transmission of a sonorous vibration.

Does this faithfully agree with the conception of its author? Here is how Prof. Bell expresses himself in the description of a patent taken out in England:

"In this arrangement, the effect that results from the vibration of the permanent magnet is to create waves in the electric current through the alternate increase and decrease of the current's intensity. This increase and decrease does not occur with the characteristic suddenness of an impulsive current, but is proportional to the increase and decrease of the density of the air during the vibration of a sonorous body."

In a memoir read before the Society of Telegraph Engineers of London, Prof. Bell again says:

"If Figs. 11 and 12 (see annexed Figs. 6, 7, and 8) be studied, it will be easily seen that simultaneous transmission of sounds of different powers and natures through the same wire cannot, in the case under consideration, alter the character of the vibrations that have produced them, as occurs with intermittent or impulsive currents. It merely changes the form of the undulations, and such change is effected in the same way as in the aeriform medium that transmits to the ear the combination of sounds emitted."

The thought of Prof. Bell receives, moreover, the same interpretation on the part of the most eminent specialists. Prof. Fleeming Jenkin, especially, in his deposition before an Edinburgh court, expresses him-

*In *La Lumière Électrique.*

self thus: "The diaphragm reproduces a facsimile of the waves of air that the voice has produced at the transmitting extremity; and such facsimile is the far-off expression best fitted to represent the operation."

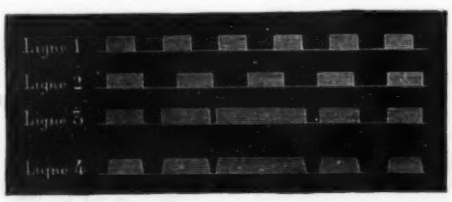
When he was asked what has been done before Bell's invention, he answered: "It had been found possible to produce the musical note, that is to say, the same number of waves per second; but the difference between musical sounds and the articulation of the human voice is that the latter depends upon the waves themselves, and their peculiar form is reproduced by Bell, and is obtained by means of the telephone."

The interpretation that I have given of Bell's idea, then, is accurate. I shall now present this question:

Upon what scientific basis is this theory of the Boston professor founded? It would be difficult, if not impossible, to answer. It is merely a bold conception that has issued from Prof. Bell's imagination. In order to have it admitted he produced his telephone, the operation of which has astonished the scientific world. The diagrams that he afterward drew, and from the analysis of which he believed he could deduce the truth of his explanations, have been admitted without discussion, and yet I shall now prove that they cannot stand a rational examination.

If Prof. Bell's theoretic ideas were admissible in a general way, and if an application were made of them in the other departments of physics, it would be necessary to adopt this formula: A physical phenomenon, during its diffusion through several media, and by different agents, must preserve all its peculiar characters without alteration in duration or form. This is an absolute negative of the law of the transformation and *reversibility* of phenomena recognized in a general way in physics. Can this formula, which, applied to the different departments of physics and chemistry, would constitute a scientific heresy, be the expression of truth in telephony? Such derogation with so general a rule would at least be surprising!

I shall now meet the learned American's first assertion. According to Prof. Bell, if intermittent electric currents are qualified for transmitting isolated musical sounds, they can in no case permit of the transmission of compound ones that give rise in the air to complex vibratory motions. The following is the shadow of a demonstration given by Prof. Bell. The example that he has selected is that of the emission of two sounds that form a major third, that is to say, the component sounds consist of vibrations whose numbers are in the ratio of 4 to 5 during the same time. There are two interrupting tuning forks interposed in the circuit of a voltaic battery that give the exact number of contacts that correspond to each note. The electric currents created by the isolated emission of each note, and the currents resulting from their simultaneous emission, are represented, according to Prof. Bell, by the diagrams 1, 2, and 3.



If the diagram is exact, an inspection of line 3 would prove that the current resulting from the major third was nearly continuous and of constant intensity. Prof. Bell, with apparent reason, concluded from it that if other sounds were to superpose themselves upon the two first, the electric line would be passed over by a constant current. There would then be no more perceptible manifestation at the receiving apparatus.

On the contrary, if each note emitted isolatedly caused the line to be traveled over by uninterrupted undulatory currents, as Prof. Bell has defined them, and as he has figured them in diagrams 6 and 7, their simultaneous emission would give rise to the production of an undulatory current whose variation in time is figured in diagram 3 of clearly characterized form. Bell concluded from an analysis of this last-named diagram that the effects produced upon the telephone receiver were precisely characterized, and must, *for this reason*, reproduce the original sonorous vibrations.

As this result cannot be obtained with intermittent currents, such as represented by Bell in 1, 2, and 3, undulatory currents, says he, are the only ones capable of allowing of the transmission of articulate sounds by telephone.

Bell's argument would be decisive were diagrams 1, 2, and 3 a true representation of the phenomena. If, in fact, it is impossible to explain the transmission of speech by *intermittent* electric currents, and undulatory ones, on the contrary, permit of the explanation of the phenomenon, it is unnecessary to seek other proof. But if, in order to obtain diagrams 1, 2, and 3, it has been necessary to have recourse to entirely gratuitous suppositions that cannot be realized in most cases, and that are not the same in the example supposed, the demonstration loses its entire value, and the conclusions fall.

Now, an analysis of these diagrams makes it clearly seen that in order to trace them it has required the three following suppositions:

1. That a current at its origin *instantly* acquires its *maximum* of intensity, and that it can end *just as abruptly*. Such a condition cannot be realized in nature, for there is always a period of increase at the closing of the current, as there is one of decrease at its termination. The various parts of diagrams 1, 2, and 3 cannot be limited, then, by verticals, but *must* be by obliques, as I show in diagram 4, which is a modification of 3.

2. Bell's second supposition is that there is but a single Voltaic battery for the two interrupters of the current. But we can just as reasonably suppose that each interrupter has at its service a distinct battery. In this arrangement, when the circuits are closed at the same time by the two interrupters, the currents from the two batteries traversing the line, their respective intensities will add themselves to each other so as to produce a durable effect upon the receiver.

The diagram that represents the electric state of the line at the time of the emission of the major third is represented at 5. It is not inopportune to point out the analogy of this diagram with that (8) of Bell's undulatory currents.

3. Finally, Bell has supposed in the third place that the durations of the closings of the circuit were precisely equal to those of the periods of opening it, for each interrupter that gives the notes of the major third. An examination of diagrams 1 and 2 leaves no doubt with respect to this. Now, such a supposition is certainly not realized in the example selected by Professor Bell. The two tuning forks connected with the line establish, in each of their vibrations, an abrupt and sudden contact with a fixed stop connected with the pole of the pile. The duration of the contact is incomparably less than that of the complete oscillation of the interrupter—a period during which there is no production of current.

The exact representation of the electric state of the line through the production of this series of almost instantaneous currents of very short duration, when each of the tuning forks is acting isolatedly, and there are two electric batteries, is shown in diagrams 9 and 10. When the two vibrators are operating together, we shall obtain diagram 11.

An examination of this diagram proves that the currents succeed one another without confusion in the line, and that each is capable of producing its effect

isolatedly at the reception. The transmission of compound sounds will result from the succession of these effects upon an appropriate receiver.

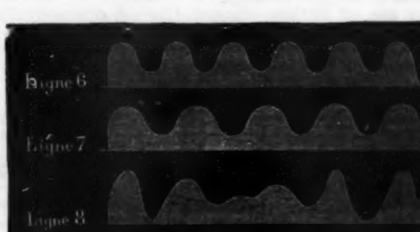
Here we are very far from the conclusions deduced by Professor Bell from an analysis of his diagrams. The only appearance of demonstration that he has given in support of his theory will not, then, withstand an elementary analysis. Will this theory further withstand an examination of known facts?

Bell asserts that intermittent currents are unfitted for the simultaneous transmission of several musical sounds through a single electric line. Gray's harmonic telegraph is the complete experimental contradiction of this assertion.

In order to avoid all discussion on this subject, I believe it well to give a succinct description of the arrangement devised by this eminent electrician.

Mr. Elisha Gray's device permits of the simultaneous transmission of several dispatches by the same conductor, in applying the property that sounds of different pitches have of transmitting themselves without confusion to different apparatus. The transmitters, like the receivers, consist of a series of vibrators. In transmission, the vibration of a strip of metal is kept up by the current from a local pile. If the lever of a Morse manipulator be pressed against the contact of the line pile, each oscillation of the vibrator will produce an instantaneous contact with a stop, and a current of very short duration will be sent into the line. The receiver consists of an electro-magnet mounted upon a sonorous box. The armature consists of a vibrating plate. There is a slide which is capable of occupying several positions in a groove at the extremity of this vibrating piece, so as to adjust the number of its vibrations to the pitch of the sound corresponding to the fundamental note of the sonorous box. The rod of the electro tuning fork transmitter of the station in correspondence is adjusted to the same note. The result of this arrangement is that the single currents produced by the oscillations of the vibrator of the sending station will be capable of causing these relays to operate synchronously, to the exclusion of the other currents which are simultaneously passing over the line and are capable of reaching other receivers whose sonorous boxes will amplify the sounds. In this way several audible dispatches can be sent simultaneously through the same wire to several distinct apparatus. Let us, in fact, imagine that at the stations in correspondence there are several vibrators attuned to different notes, with as many analyzers or receivers mounted upon sonorous boxes. If the seven Morse keys be depressed at the same moment, the line will be simultaneously traversed by the seven series of currents closed and interrupted at different intervals. It might be thought at first sight (this is the opinion of Bell, contradicted by theory and experiment) that a confusion would occur in the dispatches received; but the accordance of the vibrators and analyzers permits of the separation of these seven series of currents, and as each resonant box re-enforces only the note that corresponds to it, each series of currents finds its own receiving apparatus.

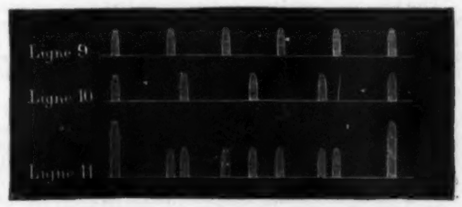
If, without in any way altering or modifying the arrangement of Gray's apparatus for the transmitters



and the line, we substitute a Bell or hammer telephone for the separate analyzers or receivers, all the distinct notes will be rendered by this single apparatus without mixture or confusion. Now, this arrangement does not modify the nature of the currents, which are *intermittent*, and not undulatory.

How is it that this experimental fact of Gray's harmonic telegraph has not struck the author of the theory of undulatory currents? It is a peremptory demonstration of the error committed by Professor Bell when he asserted that *discontinuous* intermittent currents are not adapted for the transmission of compound sounds. If, as theory and experiment prove, it is otherwise, if musical sounds emitted simultaneously can be transmitted electrically by a series of currents that succeed one another upon the line, why cannot speech be transmitted by the same means?

What, then, is speech? "Whatever be its pitch," says Mr. Jamin (*Cours de Physique*, p. 176), "the human voice is, like all sounds, composed of a fundamental and its harmonies. What distinguishes it from all sounds is the modifications that we cause it to undergo at our will, and whence results speech. Speech consists in the first place of the emission of sounds that have different characters—peculiar *timbres*—and that are called vowels. . . . The second element of human speech consists of consonants, which are not persistent tones, but modes of beginning or finishing the vowels by a sort of explosion, that is, by a movement of the air comprising at the most a very small number of vibrations of different form from those of the vowels that they modify. Such explosion precedes the musical sound of the vowel, and ceases as soon as it has taken rise in *ba, be, bi*, or else it terminates it by a final motion of the lips in *ab, at, ar*. Be-



sides this role, some consonants have the property of representing a sort of whistling or friction, *s, z, j*, which may continue indefinitely without the emission of a sound properly so called, either that the corresponding period be too rapid to make an impression upon the ear, as in the sibilants, or slow enough to permit us, as in trills, to seize the individual pulsations thereof."

It results in particular from the analyses made by Mr. Helmholtz that the vowels are nothing else than a *timbre*—an *ensemble* of characteristic simple notes that have been called vocables. They in no wise depend upon the pitch of the sound or upon the person who emits them. The vowels or diphthongs *Ou, O, A*, require but one vocable. These are respectively the notes fa_3, si_3,* si_4.*

The other vowels contain at least two vocables. They are for example:

Ai	*E*	*I*	*Eu*	*U*
re_4 and sol_5	fa_3 and si_5*	fa_3 and re_6	fa_3 and ut_5†	fa_3 and sol_3

It is undeniable—theory proves it, and Gray's telegraph renders it evident—that different musical sounds emitted simultaneously can be transmitted by a series of electric currents that succeed one another upon the same electric line. The vocables that constitute vowels are nothing else than musical sounds, and the possibility of transmitting them by discontinuous currents is therefore evident. As for consonants (explosives, sibilants, etc.), it will occur to no one that electric undulations are indispensable for the transmission of these sonorous vibrations. What remains, then, of Professor Bell's argument?

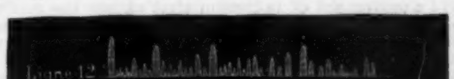
I might stop here and expatiate no further upon this point of the question, which particularly concerns electricity. Let it be permitted me, however, to rapidly cite a few experimental facts that formally contradict Bell's theory of undulatory currents.

Several successive inductions do not alter the clearness of the speech transmitted, and the interposition of an induction coil, even in a very short telephone circuit, increases instead of diminishing the intensity of the sound. Now, the intensity of induced currents depends directly upon the rapidity, upon the suddenness of the variation of the electric state in the primary circuit. It appears evident, then, that it is necessary for telephonic currents to have sudden variations, and not progressive undulations, in order that the sounds perceived through induction shall have the intensity that we find in the telephone.

In support of this assertion I shall especially cite the experimental fact that was the starting point of the researches of my compatriot Mr. Van Rysselberghe.

Remove from electric currents produced by the manipulation of the Morse key, or by the Hughes printer, their instantaneousness of production; for the direct and abrupt contact of the manipulator with the stop connected with the pile pole, substitute successive contacts with a series of springs connected with the same pole through successively decreasing resistances, so as to progressively graduate the intensity of the current that is traversing the line; this done, you will succeed in converting the previous sudden current into a true undulatory one.

What will be the result of such a modification? It will be to considerably diminish—nearly annihilate—the inducting influence of this current upon the neighboring telegraph wires, to such a degree as to render it imperceptible even in the telephone interposed in either one or the other of these circuits. This fact alone would suffice to prove that undulatory currents, that is to say, progressively increasing and decreasing



ones, traversing the primary circuit of an induction coil, are incapable of engendering in the secondary circuit that contains the telephones induced currents that are strong enough to reproduce sounds, while intermittent currents, occurring abruptly, will necessarily produce therein currents of great intensity.

I may recall, in the same order of ideas, the transmission of articulate sounds that Dr. Herz succeeded in effecting through condensers.

I might likewise dwell upon what is known to us concerning the laws of the diffusion of an electric current in an aerial, subterranean, or submarine conductor. The differences that occur in each of these cases, in time and in form, for the variable period of the electric state of the line traversed by the current, are with difficulty reconcilable with the invariability of form

* Flat. † Sharp.

prescribed by the Bell theory for the undulations of successive currents. The use of a conductor of any nature whatever does not, moreover, alter the essential qualities of the voice. The intensity alone is modified; but the articulation, which depends upon the very number and the intervals of the currents, remains perfect whatever be the composition of the circuit.

All these facts (and it would be easy to multiply examples) remove all scientific value from Prof. Bell's assertion that undulatory currents that are the facsimile of sonorous waves of vibrating air, are indispensable for the transmission of articulate sounds. I have demonstrated in an irrefutable manner that currents of very short duration that succeed one another at precise intervals on the line are perfectly fitted for such transmission.

Diagram 12, in which the engraver has brought the components too close together, gives a representation of the way in which these currents may succeed one another without confusion for the transmission of any sounds whatever—musical, articulate, etc.

The known electrical applications, the very phenomena of telephonic transmission, and all experiments relative to induction upon telegraph lines seem to demonstrate that speech is transmitted through a succession of *discontinuous currents* of very short duration, and not through undulatory ones.

It now remains for me to establish beyond controversy the truth of this conclusion, to analyze the operation of the telephone from the standpoint of mechanics, and to prove that the laws of the motion of bodies in contact are irreconcilable with the theory of the American electrician. The fact must not be lost sight of that, in the succession of the phenomena of telephonic transmission, electricity appears only as an intermediate agent, and that at the origin as well as at the reception it is merely a question as to the mode of propagating the sonorous vibrations. A study of the telephone's operation from the standpoint of mechanics is therefore of prime importance. Sound originates in a series of alternate motions reproduced at equal and very approximate intervals through the molecules, in general, of a solid, liquid, or gaseous body. A bell that is resounding communicates very lively motions to a ball placed within it. A plate that is resounding communicates motion to grains of sand strewed upon its surface. Here we are, then, led to a study of the motions of bodies, and particularly to a study of the phenomena of the shock of two bodies.

A shock occurs between two bodies that meet every time the two points through which they first came to touch each other do not in advance have equal and parallel velocities. The shock of two bodies is always accompanied with a momentary or lasting distortion of their surfaces, according as the bodies are elastic or not. In the first case the live force of the system, after the shock, remains what it was before, but in the second there is a loss of live force. The pressure that results from the contact of two spheres of masses, m and m', having velocities v and v', according to the line of their centers, will have for first effect to give them a common velocity:

$$u = \frac{mv + m'v'}{m + m'}$$

If the two bodies were wanting in elasticity, the phenomena would stop here, the contact would be permanent, and the motion would proceed with the velocity u. But if, as is almost always the case, the bodies are more or less elastic, immediately after the two spheres have acquired the common velocity the pressure that will continue to be exerted during contact (through the effect of the reaction due to elasticity) will have the effect of further diminishing that of the velocities which has already diminished, and of further increasing the other. Consequently, contact will be unable to persist between them, and there will be a separation. Let us apply this principle to the telephone. The sonorous wave strikes against a flexible diaphragm in the Bell telephone, and a thick, non-flexible block in the hammer telephone. What results from the shock? Mechanics answers: A communication of a portion of the velocity of the movable body (that is, the vibrating air) to the solid, and a reflection of the former, that is to say, a momentary separation of the two bodies. In the Bell telephone the diaphragm will bend, and make a movement toward the magnetized pole, and will afterward return to its initial position. What will the duration of this oscillation be? Prof. Bell asserts that the motion of the diaphragm will have a duration precisely equal to that of one sonorous vibration of the air. Let us discuss the scientific value of this opinion.

In the phenomena of the communication of the sonorous vibration of the air to the diaphragm we find again the elements of the shock. The vibrating air constitutes a perfectly elastic body. The diaphragm is none the less wanting in elasticity. It was immovable as regards the first body. The shock, then, causes the striking body to experience a loss of velocity and then a reflection, the velocity, on the return, being diminished by the entire quantity that has been communicated to the solid body, and which has not been restored, owing to the complete want of elasticity of the latter.

The solid body, through the effect of the shock, is submitted to a molecular concussion that it transmits, and a movement of inflection that causes the central part of the diaphragm to approach the magnetized pole. But, owing to its own elasticity, it at once returns to its first position, ready to receive the shock of the following wave. It is upon the composition, form, thickness, and mode of connection with the support, in a word, upon the body struck, and not upon the striking one, that depend the laws of its motion, and consequently the duration of the diaphragm's oscillation has no direct relation with that of the sonorous vibration of the air. If we take two different diaphragms, the duration of their motions will evidently be different. The operation of the hammer telephone very clearly confirms the truth of this reasoning. What are the constituent elements of this apparatus and their connection with the support? They are a thick piece of wood ebonite, or slightly sonorous material that the sonorous waves of the air strike; and, at the posterior surface of this, and resting upon it, a stiff lever, whose arms may be of equal or different length, long or short, without causing the articulation of the voice transmitted by the telephone to undergo any alteration.

Now, if two pendulums of different length happen to be shaken by a blow, their oscillation will be effected in periods inversely proportional to the square roots of their respective lengths. The articulation of the voice is independent of the length of the lever, that is to say, of the oscillating pendulum; and it therefore does not depend upon the rapidity of the lever's oscillation. Then the time taken by the lever armature to effect the vibration corresponding to the shock of a sonorous wave is independent of the duration of this vibration of the air. It is not equal to it, as Bell has thought it possible to assert, in neglecting the mechanical point of view, and fascinated and carried away by the apparent beauty of his conception—the uniformity of undulatory motion.

But if the duration of the lever armature's motion has no relation with that of the sonorous vibration of the air, the duration of the electric current which results therefrom is likewise independent of that of the sonorous wave. The phases presented by the currents that succeed each other in the line are not identical with those of the vibrating air. The intensities of the electric currents are not, at every instant, as Prof. Bell says, proportional to the density of the vibrating air. Prof. Bell's theory has no longer any basis; it is contradicted by the elementary principles of mechanics.

Let us pursue the study of the hammer telephone, and a flood of light will be shed upon the laws that preside over the transmission, the electric diffusion, and the telephonic reproduction of sounds. Is there in the hammer telephone a momentary separation of the parts through the effect of the sonorous shock? Experiment shows that this apparatus permits of the transmission of speech without the aid of an electric battery, through a simple metallic connection with a similar or Bell telephone used as a receiver. The lever armature undergoes oscillatory motions, then, opposite the pole of the magnet, when sounds are emitted in front of the solid piece upon which it bears through one of its extremities. The principles that we have above adduced establish that with elastic bodies there is an independence of motion between the striking body and the one struck. The same independence exists between the motion of the body that serves as an intermediary to the transmission of the shock and that of the lever armature.

The physical phenomenon of ivory balls suspended in a row in contact with one another, and which under the action of a blow communicated to the first of the series all remain immovable save the last, perfectly explains what occurs in the telephone. It results from this fact that even in case the two pieces in contact were of the same nature, material, and form, and had the same mobility, the intermediate piece would remain apparently immovable under the action of the sonorous shock, and the armature alone would have a real motion. But in the telephone, besides, the nature of the two bodies is different, and their modes of connection with the support are different. With the intermediate body all inflection is practically impossible. The separation of the two bodies is evident.

The sonorous blows communicated to the thick piece in the hammer telephone produce the same effect in the lever armature as the vibrations of a bell do upon a ball placed within it, as the vibrations of a resonant plate upon grains of sand scattered over its surface.

When a carpenter wishes to drive a nail into a partition, he has his assistant exert a stress with a piece of wood at the corresponding point on the opposite side. The man who performs this function knows perfectly well that whatever be the reaction that he wishes to oppose, whatever be the amount of stress really exerted, the effect of each blow of the hammer is to cause a motion of the block of wood, and a breakage of its contact with the partition. Just so, when the sonorous wave strikes the piece of wood or ebonite upon which the extremity of the lever armature bears like an anvil, the magnetic attraction of the telephone magnet is powerless to keep up the contact. A separation must occur. The velocity communicated to the movable piece by this force of instantaneous duration is soon destroyed by the continuous action of the magnetic force that brings the armature back to its initial position.

As for the intermediate piece, if even we suppose that it is capable of effecting a real motion (as it has no connection with the hammer of the lever), it is its own elasticity that leads it back to the former state. The reactions that govern the motions of the two organs are, then, essentially different, and the motions that result from them must be so likewise. It is not possible to oppose a *scientific argument* to this explanation, which rests upon a rigorously exact basis. A denial can doubtless be made, but a simple denial cannot prevail over certainty.

The entire experimental study of the hammer telephone is in accordance with these theoretical deductions.

The following are a few demonstrative facts:

When we fix the armature to the solid piece, the apparatus no longer operates.

The use of flexible diaphragms considerably diminishes the intensity of the sounds transmitted.

If we fix the armature to the magnet, and, if we even confine ourselves to establishing a physical contact of the armature, by interposing materials deprived of magnetism, we stop the operation of the apparatus.

On another hand, the reproduction, of sound is not perceptibly altered on reception, when between the solid piece and the hammer we interpose deafening materials, such as blotting paper, cloth, damp kid skin, etc.

The strong sounds emitted by the telephone cannot certainly be attributed to the inflections of these non sonorous substances. In order that there shall be a production of sound, it is in all cases necessary that there be a series of hammer blows upon the anvil, and the effect of which cannot be annulled by these deafening materials.

As a last conclusive fact: when the telephone currents that traverse the apparatus are strong enough (like those given by the vibration of a spring upon the support of the pantelephone), we perceive perfectly, upon placing a finger upon the front of the telephone, the tremor of the hammer that is striking the rigid piece.

I now sum up and conclude. From the analysis that I have just made of the operation of the hammer telephone there result the following conclusions, viz., that through the emission of sounds, as for the production of them, the lever armature undergoes genuine motions; that the duration of each of these oscillations has no direct relation with that of the sonorous vibration of the air; that these motions, then, cannot be facsimiles of the sonorous waves of the air; that the electric currents produced by these motions do not depend, as regards their duration and intensity, directly upon the duration and amplitude of the armature's motion, and cannot have duration or phases equal to those of the sonorous vibrations of the air; that these currents are not, then, undulatory ones answering to the definition and explanations of Prof. Bell; in a word, that Prof. Bell's theory is not applicable to the operation of the hammer telephone.

I have, moreover, in the course of this article, contrary to Prof. Bell's assertion, established the possibility of transmitting several simultaneously emitted sounds through a series of intermittent currents of short duration. Gray's harmonic telegraph has permitted me to prove by experiment the truth of my theoretical demonstration.

An analysis of the subject, regarded from mechanical and electrical standpoints, shows that the telephonic transmission of sounds of every nature whatever—musical, simple, compound—is produced by a series of *discontinuous* currents that succeed one another upon the line. These currents are exactly equal in number to the sonorous vibrations of the air corresponding to each one taken isolatedly. They succeed each other at intervals precisely equal to those that separate such vibrations. Their intensity is directly proportional to that of the sonorous vibrations.

Such is the rational explanation of telephonic phenomena, and the only true one, since it alone is in accordance with scientific principles and the results of experiment.

The abrogation of the law recognized in physics does not, as Prof. Bell has supposed, exist.

Telephonic transmission by electricity obeys the general law of the reversibility of the phenomena of nature, which, after a series of successive transformations, in which their peculiar characters and phases may be absolutely modified, reappear with all their constituent qualities.

The original sound is produced by mechanical action. In its propagation through the air, and then through the solid parts of the telephone, the mechanical motions are effected with the qualities proper to each of those media. Mechanical motion is afterward partially converted into magnetic or electric motion of several natures, through one or several inductions, in order to become again magnetic and then mechanical, with phases in the receiving telephone contrary to those that it had in the transmitter.

THE STREET AND MAQUAIRE LAMP.

HERE at length is a lamp that does not resemble all the rest. To describe a regulator is always a drudgery.

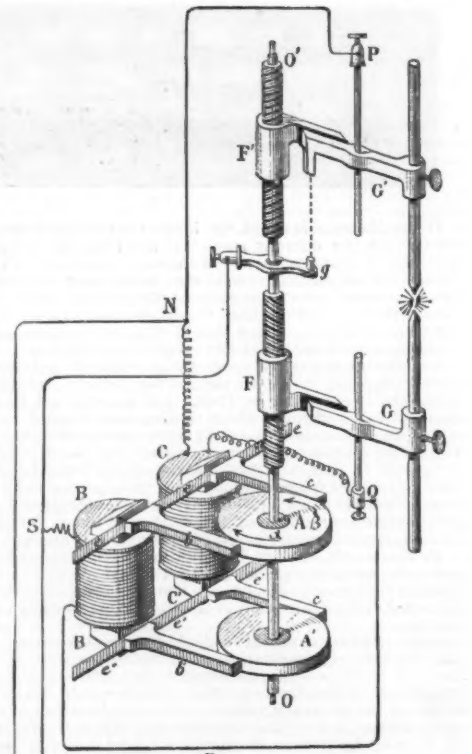


FIG. 1. THE STREET AND MAQUAIRE ELECTRIC LAMP.

Doubtless a new point, an ingenious detail, is found, and it is for that reason that the apparatus is brought to notice and described; but, in order to reach such interesting point, one finds himself obliged to again pass through the nomenclature of parts—solenoids, clockwork, etc. To-day we get out of the rut, for Messrs. Street and Maquaire's lamp differs both in principle and arrangement from all the systems conceived for the same purpose.

In the first place, all, or almost all, the lamps that are known merely obtain from the current a regulation of the carbons' motion, the motion itself being due to a non-electrical cause. In the great majority of cases, gravity is the motive power. The only lamp that we can mention in which the approach or recession of the carbons is effected by the current itself is that of Mr. Tchikoleff.

The Street and Maquaire lamp proceeds in the same manner, but the motion is brought about in a particularly ingenious and new way. In order to cause the carbons to move under the influence of the current, it is necessary to devise a small electric motor, and this is what was done by Mr. Tchikoleff, who simple borrowed the Gramme ring.

The lamp under consideration is constructed for running especially with alternating currents, which, as well known, are but slightly adapted for putting the

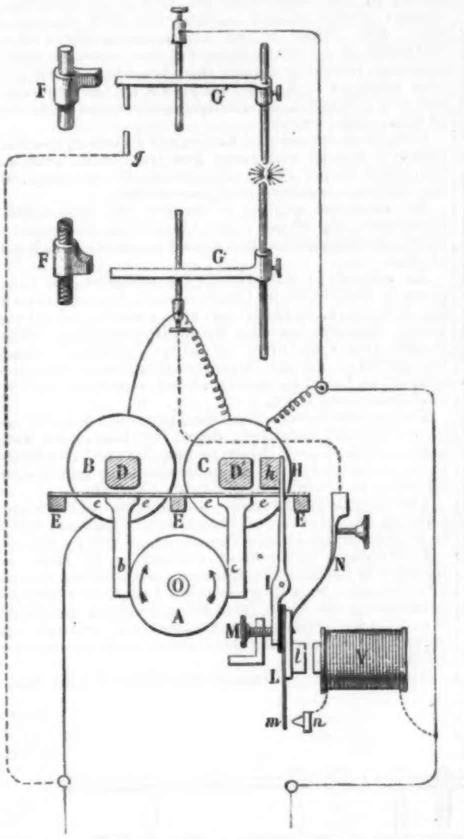


FIG. 2.—THE STREET AND MAQUAIRE ELECTRIC LAMP.

part in motion. By a curious detour, it is precisely by relying upon this very alternation that the inventors have effected their regulation. These gentlemen, who are known as the engineers of the Sun Lamp, were led by reason of their occupation to make a particular study of alternating currents, and, during the course of such labor, were led to a special consideration of the vibrations that these currents may give rise to. We can, in fact, by sending an alternating current into an

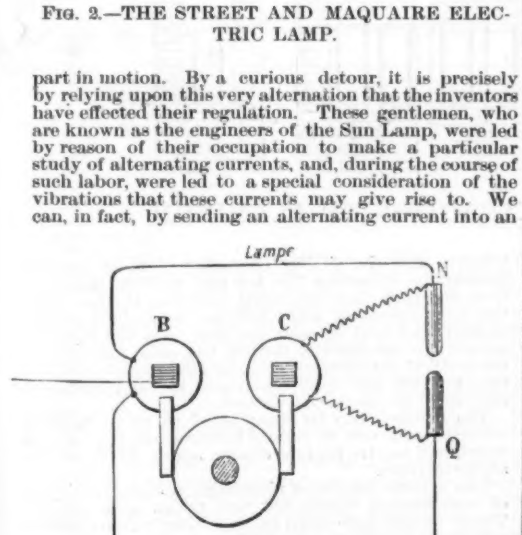


FIG. 3

electro-magnet, put into and keep in vibration a spring armature placed before the core of the electro. The armature, the spring, and the electro itself must be placed in certain conditions that correspond to the duration and intensity of the current's phases. If such conditions are fulfilled, the armature will be attracted every time the current passes through a maximum. Every time the current is annulled, in order to change direction, the abandoned armature will obey the spring, and recede.

Messrs. Street and Maquaire have devised a very simple motor from this vibratory motion. Let us imagine that the armature thus set in vibration is provided with a click that rests upon the toothing of a ratchet wheel. At each of its vibrations it will cause the wheel to move forward one tooth, and the wheel will take on a continuous rotary motion. But, as may be conceived, such a motor would be somewhat delicate. In fact, the oscillation of the tooth would be very rapid—something like seven or eight thousand vibrations per minute, and the amplitude by compensation would be very slight. The ratchet wheel, then, would have to have exceedingly fine teeth, and it would be difficult to keep the click in perfect contact with it.

Another ingenious idea will serve to correct the trouble. Let us make the click and wheel of soft iron, and what will happen then? At the moment the click (which forms part of the vibrating armature) is attracted by the electro-magnet, it will be magnetized. At the moment the electro abandons it, and it comes under the action of its spring again, it will be no longer magnetized, and it follows that, in its forward motion, it will adhere to the wheel, and that in its backward it will not. This is a very simple mode of securing a contact of the click with the teeth, or rather, what is better, it is a means of entirely suppressing the teeth. In fact, what good are the teeth? Magnetic adherence ought to suffice, and indeed it does suffice perfectly.

Messrs. Street and Maquaire have applied the motor thus sketched, as follows: As we are making use of alternating currents, the two carbons will wear away equally. It is a question, then, of causing them to advance toward each other equally and with proper velocity. To effect this the two carbon supports, GG', form the nuts of a screw, OO', having two contrary threads. When the screw revolves, the two nuts and the carbons that they carry will recede from or approach each other. Upon the axis of this double screw there is fixed an iron disk, A. In reality there are two of these disks, A and A', but as their actions coincide, we shall suppose provisionally that there is but one. At the two sides of this disk, and at the extremities of the same diameter, there are two pieces of iron, *b* and *c*, tangent to the disk and mounted upon a small spring, *ee*. In front of the extremities of these two pieces, *bc*, are arranged two electro-magnets, BB' and CC'. These latter are both connected with the general current, and one of them, BB', is placed in the lighting circuit, as shown by the line, M, N, P, G', Q, R, B', B, S, T. This electro is provided with a few spirals of coarse wire. The other, CC', is placed upon a derived circuit taken at the terminals of the lamp at M, N, C, C', Q, R, B', B, S, T, and is wound with fine wire. This arrangement is the one that is applied to differential lamps.

The apparatus operates as follows: Let us suppose that the two carbons are in contact. The resistance of the circuit, M', P, Q, S, T, is very feeble, the entire current is directed therein, and the electro, BB', is strongly excited. The strongly magnetized contact piece, *b*, acts energetically upon the disk, M, and causes it to revolve in the direction shown by the arrow, *a*. The screw follows the motion, the two carbons recede, and an arc is formed. The current weakens at the same time in the electro, BB', but increases in CC', and the piece, *c*, adheres more and more strongly to the disk, A, and tends to make it revolve in the direction of the arrow, *β*. There is an equilibrium for a certain time, and then the carbons recede as a consequence of the combustion. The electro, CC, takes its place above, and a revolution in the direction *β* is brought about and goes on continuously, compensating at every instant for the wear, and keeping the current strictly at the same intensity. The system, A', *b'*, *c*, placed below the axis, repeats the upper system, and doubles its action.

The running of the apparatus is absolutely regular. The little motor, AA', revolves gently and continuously without showing what moves it, the vibration of the tangential pieces being very rapid. Sometimes, if, through the homogeneousness of the carbons, the arc has a tendency to abnormally lengthen or shorten, we observe the small disk now to hasten its speed, now to slacken it, and now to stop; to move for a moment in an opposite direction, and then, all being in order again, to resume its regular operation. The light is excellent, as may be readily conceived, and all inequality due to mechanism has disappeared.

The inventors have provided their lamp with accessory arrangements, in view of different contingencies that may arise. First, if the carbons become entirely consumed, one of the holders, G (Figs. 1 and 2), in its motion touches a contact, *g*, and closes a short circuit that cuts the lamp out of the circuit. On another hand, the lamp may be extinguished by accident, in which case the entire current would pass through the solenoid, C *c'*. At the same time that the general operation would be interfered with, the solenoid might be destroyed. Such an accident is prevented by the arrangement shown in Fig. 2. At the moment the lamp goes out the strongly magnetized electro, C, attracts the armature, *h*, and tilts the lever, H, L. In this motion the rod, *m*, touches the contact, *n*, and thus forms a derivation through the bobbin, V. This latter has a resistance about equal to that which the arc normally had—or rather a little greater. It performs two functions; on the one hand, it keeps up the contact, *mn*, by attracting the armature, *l*, and preserves its current; and, on the other, it lightens the electro, C, of the excess of current that might injure it. It preserves enough of this, however, to cause the disk to revolve so as to bring the carbons together. When these latter come into contact, the circuit that they formed having almost no resistances, the entire current, abandoning the electro, C, and the bobbin, V, flows into them. The lever, HL, actuated by its spring, N, returns to its normal position, while the electro, B, separates the carbons, and relights the lamp.

Instead of placing this arrangement in the lamp itself, the inventors prefer to have it separate therefrom, and they therefore give it the form shown in Fig. 3. During a normal operation, the current entering at P traverses a small solenoid, A, which keeps lifted a metallic fork placed over two mercury cups, *m m'*. After passing through A, the current traverses the lamp and makes its exit through the circuit, T, S, R. If the lamp goes out, the current tends to weaken, the solenoid, A, abandons the fork, and the latter falls into the cups *mm* and reunites them in short circuit. The current from P then finds two paths; one leads it as before to the lamp, and the other allows it to pass through a bobbin, B, of a resistance almost equal to that which the arc had. It will divide itself between these derivations, and the portion passing through the lamp will permit the electro, C, to bring the carbons together, while the other, which passes through B, will assure of the service in the balance of the circuit, and prevent accidents. When once the carbons are in contact, the bobbin, A, will raise the fork, and the relighted lamp will begin its operation again. This device will act not only in case of accident, but also in case of voluntary extinction.

To eulogize the operation of this lamp at the moment at which it has just been devised, at the time when it as yet exists only in an experimental stage, would doubtless be hazardous, and we know that sufficiently prolonged experimentation can alone permit of pronouncing upon it.

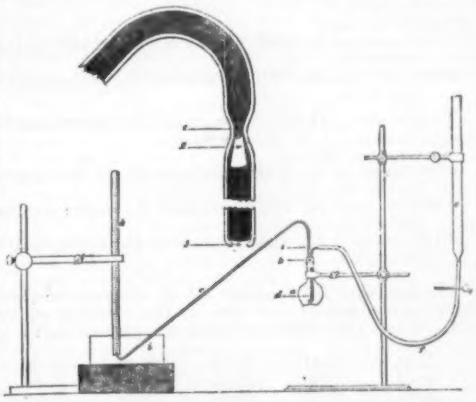
We can already see, however, that the new apparatus is possessed of great simplicity, and that this assures us of a strong and cheap lamp. These are features of great importance in an apparatus of this kind, and when we find them at the very inception of a system we can risk predicting a fine future for it.—*La Lumiere Electrique.*

APPARATUS TO DETERMINE EQUIVALENTS.

By H. N. MORSE and E. H. KEISER.

THE design of the first year's course of laboratory work for undergraduates in this university is to teach the important facts of general chemistry. During that time analysis is taught only incidentally. The course begins with a series of exercises in manipulation and in the calculations incidental to quantitative chemical work, each exercise being preceded by a lecture, in which the subject matter is explained or illustrated, and the student warned against the errors which he is likely to make. This division of the course includes such exercises as: the manipulation of glass; the setting up of apparatus; the various operations connected with the solution and the precipitation of substances; the collection and washing of precipitates; the use of the blow-pipe; recrystallization; sublimation; distillation; the use of the balance; the measurement of liquids and gases; and the correction of gas volumes to standard conditions of temperature and pressure.

The student, having thus acquired some understanding of laboratory processes and some skill in manipulation, begins to experiment upon the elements and their compounds—first of all upon hydrogen. Throughout the remainder of the course much attention is given to enabling the student to demonstrate for himself the quantitative relations of the various substances with which he works. Great care is taken, however, to keep



APPARATUS TO DETERMINE EQUIVALENTS.

out of his hands such apparatus and such methods as give results which approximate only roughly to the true ones; because results of this character are believed to be not only destitute of educational value, but positively demoralizing to the student. The determination of the equivalence of the elements by the students is naturally regarded as very important, because that is the fact on which most of our chemical reasoning is based. In connection with his first piece of experimental work the student determines the equivalence of some of the metals in terms of hydrogen. For this purpose the simple apparatus here described is employed. The results obtained with it have been so generally and so thoroughly satisfactory, that we venture to publish a description of it for the benefit of those who may be following a similar course of laboratory instruction.

The apparatus consists of:

a. A flask having a capacity of 40 or 50 c. c.

b. A rubber stopper having two holes. The stopper should fit the neck at all points so tightly that no gas can lodge between it and the glass.

c. A small glass delivery tube of the form indicated, which ends at the lower surface of the stopper. The upright portion of this tube is somewhat contracted at 1, as shown in the enlarged figure. A rather tightly fitting conical roll of platinum foil is placed at 2. The remainder of the tube between the platinum roll and 3 is filled with glass wool. The opening at 3 is narrowed down, after the glass wool has been introduced, by allowing the glass to soften in the flame.

d. A second glass tube which nearly touches the bottom of the flask and turns up at the end. The opening of this tube is also contracted.

e. A glass tube holding about 100 c. c., which serves as a reservoir.

f. A small rubber tube with thick walls, which connects *e* with *d*.

g. A Mohr's pinch-cock, which serves to open or close *f*.

h. A calibrated, 100 c. c., gas measuring tube graduated to $\frac{1}{10}$ c. c.

i. A glass dish.

The apparatus can be employed to determine the equivalents of all those metals which liberate hydrogen in equivalent quantities when treated with acids or caustic alkalies; provided, of course, such metals can be obtained in a sufficiently pure condition. The

metals which we usually employ as examples are zinc and aluminum.

The experiment is conducted in the following manner:

e is filled with distilled water; a piece of zinc weighing from 0·150 to 0·200 gramme is placed in the flask; the pinch-cock, *g*, is then opened, and the whole apparatus allowed to fill with water. The apparatus is now examined in order to ascertain if gas bubbles are lodged under the stopper, *b*, or in the glass wool. If so, they can usually be dislodged without difficulty. If they persist, a few moments' boiling of the water in the flask will effect their complete removal. Having thus effected the complete removal of the air from the apparatus, the eudiometer is placed over the outlet of the delivery tube, and the greater portion of the water remaining in *e* allowed to flow through the apparatus. Sulphuric acid of the concentration ordinarily employed in the laboratory (1 of H_2SO_4 to 4 of H_2O) is poured into the reservoir, *e*, until it is nearly full. The pinch cock, *g*, is then opened, and the water which fills the apparatus is displaced by sulphuric acid. The action of the acid upon the metal may be facilitated by heat or by adding some platinum scraps. When the action is over, the contents of the flask are swept through the delivery tube by again opening the pinch-cock, *g*. Finally, the eudiometer is transferred to a cylinder of water, the volume of the gas read and corrected in the usual manner. If hydrochloric instead of sulphuric acid has been used, which would be the case when the metal employed is aluminum, a little caustic soda should be added to the water in the cylinder to which the eudiometer is transferred.

We give below a number of examples of the results obtained by our students during the present academic year.

A. obtained from

0·1045 gramme Zn, 0·003223 gramme H_2, or the equivalent 32·42.

0·1070 gramme Zn, 0·003306 gramme H_2, or the equivalent 32·37.

B. obtained from

0·07475 gramme Zn, 0·00229 gramme H_2, or the equivalent 32·53.

0·0729 gramme Zn, 0·00224 gramme H_2, or the equivalent 32·56.

C. obtained from

0·1098 gramme Zn, 0·003375 gramme H_2, or the equivalent 32·54.

0·1400 gramme Zn, 0·004307 gramme H_2, or the equivalent 32·50.

D. obtained from

0·1180 gramme Zn, 0·00364 gramme H_2, or the equivalent 32·40.

0·0925 gramme Zn, 0·00283 gramme H_2, or the equivalent 32·62.

E. obtained from

0·1025 gramme Zn, 0·003167 gramme H_2, or the equivalent 32·36.

0·0610 gramme Zn, 0·0018852 gramme H_2, or the equivalent 32·36.

To test the working of the apparatus, three experiments were made by ourselves:

We obtained from

0·1959 gramme Zn, 0·00602 gramme H_2, or the equivalent 32·55.

0·1646 gramme Zn, 0·00507 gramme H_2, or the equivalent 32·48.

0·1436 gramme Zn, 0·004419 gramme H_2, or the equivalent 32·49.

The following list contains all of the results which have been obtained with zinc by the members of our class of first year students during the present year:

32·34	32·44	32·36	33·20	32·30
32·63	32·50	32·55	32·30	32·40
32·66	32·54	32·38	32·80	32·57
32·60	32·53	32·45	32·70	32·42
32·80	32·37	32·46	32·40	32·41
32·30	32·56	32·32	32·60	32·40
32·50	32·87	32·62	32·20	32·20
32·34	32·36	32·36	32·50	32·30
32·69	32·63	32·60	32·20	32·40
32·58	32·37	32·56	32·62	32·37
32·42				

The results obtained with aluminum are not less satisfactory, but enough data have already been given to demonstrate that the apparatus answers very well the purpose for which it was devised.

We use the same apparatus, with some modifications which will be described hereafter, for the determination of the commercial value of zinc dust.

Johns Hopkins University, Nov., 1884.

—*Amer. Chem. Journal.*

CONVERSION OF LIQUID FAT ACIDS INTO SOLID PRODUCTS.

THE method of St. Cyr Radison (of Marseilles) for converting oleic acid, a by-product of the fat industry, into solid palmitic acid rests upon the reaction discovered by Varrentrap in 1841, treatment with fused caustic potash in large excess, whereby potassium palmitate and acetate and free hydrogen are produced.

$$C_{18}H_{34}O_2 + 2KOH = C_{16}H_{31}KO_2 + C_2H_3KO_2 + H_2.$$

The operation is carried out in a decomposing vessel of wrought iron with cast iron bottom heated over an open fire, with a large fire space in order that the necessary temperature may be uniformly maintained.

In the process of W. L. Carpenter, a closed pan provided with manhole, safety valve, gas exit, tube, and stirrer is used. The gaseous products are passed through a coke scrubber and thence into a holder. The charge is 1,500 kilos. oleic acid and 2,800 KHO solution, sp. gr. 1·4. Decomposition begins at 290° C.; the most favorable temperature for the operation being 300°—310°. At 320° destructive distillation begins, and steam is blown in as a preventive measure when this temperature is reached. The process lasts 36 to 40 hours. The end of the reaction is ascertained by testing the fusing point of the palmitic acid produced, using the method of Dalican.—*Ding. Pol. Jour.*

ALUMINUM AND ITS EXTRACTION ON A COMMERCIAL BASIS.

By F. J. SEYMOUR.

THE metal aluminum has such peculiar and desirable qualities, especially in its use as an alloy with other metals, that the writer in the year 1878 made some practical experiments looking to its production, which consisted mainly in the attempt to separate the metal from commercial zinc, having found that the Missouri and Illinois product carried aluminum in a small percentage; and for this purpose he constructed a furnace by which the metals of greater or lesser specific gravity could be separated while in a molten state, relying upon the difference in their specific gravity, also upon the degree of heat at which each metal under treatment began to crystallize. From this period of time the first recognition of the fact that natural affinity between the metals aluminum and zinc would sooner or later develop the economical process for their reduction became apparent. First experiments were made under the belief that the two metals could be reached together by distillation with appliances analogous to the distillation of zinc ores, and that the two ores, by treating together with carbon and other fluxes, could be cast down as an alloy in a metallic condition. Partial success was reached in these experiments, but it became apparent that the conditions for successful treatment should include an arrangement so complete that the very ethereal vapors distilled from the ores must be held under more rigid conditions than were needed for the distillation of zinc ores alone; and after repeated careful experiment on a practical basis, he finally determined that the economical process for obtaining the metal aluminum would be found by a process of vaporization and condensation, and under such condition as would hold under proper control the vapors liberated and almost entirely excluded from contact with the atmosphere, also subject to such conditions as would allow the introduction of hydrocarbon and other gases at any time needed to make perfect reduction of the silica and impurities, thereby making such concentration as would deliver the mineral contained in the ores in the condition of their oxides at one point, and the impurities to be delivered at another, and to be done by such an arrangement of appliance as would include cheap labor and the handling of large numbers of retorts and large quantities of material.

Early effort to obtain this metal may be said to have commenced about the year 1855, since which time Messrs. Bell & Co., Wurtz & Co., DeVeile, Wholer, and later James Webster and Freishmuth of our own country have each made success in obtaining the metal, but at such cost as to exclude its use for common purposes, and thereby it has been out of reach for those purposes for which its inherent qualities render the metal peculiarly adapted.

My process for obtaining aluminum is substantially as follows, and consists in taking the natural earths carrying the metal, and treating them with calamine or carbonate zinc ores and other fluxes.

My formula is substantially as follows: Zinc ore 100 parts, kaolin 50 parts, carbon either as anthracite coal or its equivalent in hydrocarbon gas 125 parts, pearlash or its equivalent 15 parts, chloride sodium or its equivalent 10 parts, and may be varied somewhat, all these parts (except hydrocarbon gas when used) to be intimately mixed together by grinding under heavy rollers.

It has been found that the earths or ores peculiar to America carrying aluminum are mainly composed of silica and alumina in about equal proportion, viz., on average 50 per cent. alumina and 45 per cent. silica, the balance of parts being moisture and impurities. The union of the alumina and silica as found in the natural earths is a mechanical not a chemical one, and it has also been found that all attempts to cast down the metal from the natural earths directly or by any process analogous to the general method of metallurgic operation, or to the method generally in use for the distillation of zinc ores, have failed for the reason that alumina in its natural condition as found in the various earths is so very highly sublimated, also so very light in its specific gravity when reduced into a metallic condition, that all the common methods of treating by heat either in an open crucible or by distillation will fail to cast down in any way analogous to the common methods of treating ores.

The method herein described consists mainly in the use of zinc ore of the calamine or carbonate order with kaolin or earths carrying alumina, for the reason that the one metal has such chemical affinity for the other, and the one such greater specific gravity than the other, that when mixed in proper proportion and subjected to heat under conditions as hereinafter explained, the one will act as a flux, and assist in liberating the oxides to be taken down together either as the oxides of the two metals or in a metallic condition.

The deposit of alumina in all the rocks and earths is found in such sublimated condition as to approach more closely than most other deposits of mineral to what may be stated as condensed or congealed atmosphere, and under a moderate degree of heat is expelled in such rarefied vapor as to escape observation.

A quantity of kaolin placed under heat in open crucible (showing 40 per cent. alumina by critical analysis) will before the silica is calcined have entirely disappeared, so that no trace is found by the same analysis as the one preceding its being placed under heat.

Many inquiries have been made attempting to determine from what source the great deposit of alumina projected. Geologists determine that one-eighth per cent. of the entire crust of the earth is an aluminous deposit. There are certain localities and special formations of the earth's surface that show in excess the deposit of this mineral, notably the drift clays in America. The valley of the Connecticut River shows a drift clay of ancient origin, yet the deposit is still progressing, resulting mainly from the abrasion of granite and feldspar rocks carried down by the current and left as a sediment, accumulating since the early history of this notable river.

Extensive deposits of kaolin are found in the State of Georgia, almost white and free from iron. This kaolin will show 40 per cent. aluminum, 47 per cent. silica, the balance moisture and impurities.

An immense deposit of kaolin clay is found in Virginia, carrying 60 per cent. aluminum and 9 per cent. silica, the balance moisture and impurities with a trace of iron.

An abundant supply of clay adapted for the purposes of treatment by this process is found in the State of New York, notably in the vicinity of Syracuse. Large deposits are also found in some of the Western States and Territories, notably Colorado; generally the kaolin of all the deposits mentioned, except that noticed as Virginia, shows about equal proportions of aluminum and silica.

The process herein described proposes to treat kaolin in its natural condition direct by heat, and includes making use of such fluxes as may be used to advantage in its reduction and concentration and delivering it either in the condition of oxide or metal.

The character of furnace and method of working is as follows. (An accompanying sketch of furnace will help to explain more clearly the nature of the process.)

Distillation under heat carried up to that degree which will liberate the mineral contained in the kaolin or other ores in vapors, conducting the vapors into a condensing chamber, from thence taking the condensation automatically down and out into settlers, easy of access for subsequent treatment and reduction into metallic aluminum.

The analogous chemical properties of zinc and aluminum, also the similar character of their natural depositions, including the low and uniform degree of heat at which they are fused, and from the fact that the same agents or acids will act upon both metals equally, and hold in solution, also that the same reagents will precipitate, furnish the basis upon which my method of treatment and also the character of the fluxes which are used contribute to the measure of success which has been attained.

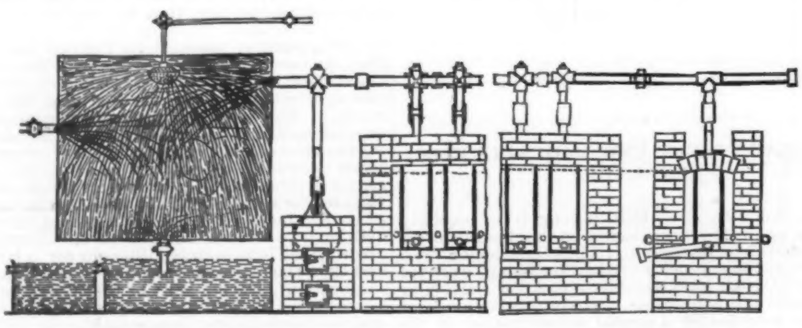
The furnace may be constructed so as to carry in number from one to several hundred retorts, each of which will receive for one charge about 100 lb. of the above mixture.

The retorts may be of plumbago, preferably of steel of such temper as will not fuse under 4,000 deg. F. These retorts are coated on their inner surface with a refractory material, preferably quicklime and borax in equal proportion held in solution of that degree of plasticity that the coating may be uniformly laid upon the inner surface.

The retorts are, inside dimensions 36 inches long, 12 inches diameter, with sides, ⅝-in. thick. They are set vertical in a furnace chamber separated by about four inches from contact with any other of the same series; the chamber to be heated preferably by gas to insure great uniformity of heat, which should be carried continuously at about 2,500 deg. F.

The degree of heat applied and the uniformity with which it is conducted will determine the length of time needed to effect complete reduction of the ores.

Properly handled, the furnace should make two charges in 24 to 30 hours. The retorts are so arranged that access may be had to the contents at the bottom for a two-fold purpose, one to reach and remove the ashes and cinders occasioned by the reduction, and also to obtain any metallic deposit resulting from the action of the heat and fluxes; the other purpose, to be enabled at any time to project hydrocarbon gas into the retorts and through the contents, thereby giving direction to the vapors liberated into the condensing chamber, making one of the essential conditions of having under control the amount of heat within the retorts, also determining to a great extent the character of and rapidity with which work may be accomplished. The retorts are also provided at their upper end with an adjustable cap or cover, fitted so as to be centrally placed thereon, also fitted centrally with an outlet pipe connecting with a main pipe leading into a condensing chamber. Each retort is also so arranged as to be operated singly or in connection with a series of retorts placed in the same furnace. They are also so arranged as to be charged with fresh material without disturbing the proper working of any other one of the same series. The condensing chamber to which the vapors are conducted should be, for a furnace of 10 retorts, about 8



APPARATUS FOR THE EXTRACTION OF ALUMINUM.

feet diameter and 8 feet long, of thin copper fitted with proper connection to the pipe to which each retort has access, also provided at its outer end with an outlet pipe provided with a valve. The condenser should also be provided with an inlet pipe at its uppermost surface, projecting into the condensing cylinder for sufficient distance to allow space for a perforated cap through which is to be projected into the condensing surface water or other fluid in a finely diffused spray. The condensing cylinder is also provided with an outlet pipe at its extreme lower surface. This pipe is also provided with a valve to regulate the flow from the condensing cylinder, through the outlet pipe, into a series of settlers as fast as condensation has taken place.

The furnace is also so arranged that one or more of the retorts may be charged with a varying mixture of kaolin and fluxes, or one or more may be charged with flux alone, while others of the same series may contain zinc or other volatile metal; while the vapors liberated are all united and brought into contact with each other either on the way to or in the main condensing cylinder. This arrangement is advantageous for the reason that the characters of the ores obtained from different localities are dissimilar in their properties, and require change in treatment, and this arrangement of furnace will enable me to treat any ores that carry aluminum, and is intended to treat the natural earths, although the oxide of one or both may be substituted and treated by this process with fair results. This furnace is also provided (in addition to facilities for a supply of hydrocarbon gas to be forced through the retorts and their contents while under heat) with an auxiliary furnace for the purpose of developing any gas (and its introduction into the main pipe leading to the condensing cylinder) that will assist in the condensation of the vapors that are liberated by heat and forced into the condensing chamber. In fact, this furnace and its arrangement are well adapted for extracting and concentrating the mineral from any earth or ore carrying a volatile metal which fuses at a low degree of heat. This furnace will also treat with fair success most of the kaolin clays, and with proper fluxes, and a proper degree of heat, make such concentration and deliver the oxide of aluminum nearly separated from the silica and impurities, and in such condition as to be readily absorbed by metals in a molten state, notably iron and steel, copper, and zinc, making thereby an alloy with those metals more readily absorbed by the metal for which it has the greatest affinity, also the one fusing at about the same degree of heat, notably zinc.

Four men should operate fifty retorts for twenty-four hours and proper appliance for making the mixture of kaolin and fluxes, charging each retort as the proper amount of reduction has been reached from time to time as required to keep the whole number in process or work and make the operation continuous.

Under the formula hereinbefore stated, and fairly handled by the furnace and process described and resulting therefrom, should be found in the settlers the oxide of zinc and oxide of aluminum in proportion almost exactly corresponding to the proportion with which the two ores were mixed and placed in the resort.

I have found it advantageous to use zinc ore in excess over that of aluminum ore, and for the reason that actually the zinc begins vaporization first, and at about 200 deg. F. below the point at which the alumina commences vaporization from the kaolin, and hence the advantage of having and using appliances by which complete control may be had over the contents of the retorts, and by observation and regulation, or adjustment, of the valve on the outlet pipe of the condenser, one can determine at all times the character of the condensed oxides.

The metallic cover for each retort is fitted so as to be luted into place after the retort had been charged, and also may be weighted when in place to insure, in connection with the valves projecting outward from the condensing chamber, the proper amount of working pressure upon the contents of the retorts while under heat; for it will be observed that until the moisture carried by the ores and fluxes into the retorts has been expelled, greater outlet for the escape of moisture and superfluous gases will be proper, which results may be obtained by the arrangement of furnace as described and shown.

The kaolin clays alone with modified arrangement of fluxes, and submitted to the proper degree of heat, may be concentrated to that extent as to make almost complete separation of the alumina from the silica.

The process of taking down the oxides into metallic form as one alloy of zinc and aluminum is simple and not troublesome. The separation of the two metals is also simple, and may be made without appreciable loss of either.

THE CLAPP-GRIFFITHS STEEL PROCESS.

THIS new process of Messrs. Clapp and Griffiths is a pneumatic system in many respects similar to the Bessemer. The main points of difference are in the stationary converter and the position of the tuyeres. These are in the sides instead of the bottom of the vessel. At first sight these differences appear very small, and one is inclined to doubt the statement that the merit of the new product depends entirely upon the design of the converter; yet, on investigation, such appears to be the case.

Mr. Griffiths was the engineer-in-chief of the Gilchrist-Thomas process during its experimental period. He afterward devoted himself to developing his present principle. His associate, Dr. Clapp, was a physician of some prominence in one of the large iron districts of England. He had obtained patents for a process of his own, which stood in Mr. Griffiths' way. This difficulty was very wisely met by a consolidation of their interests. Among the first converters in operation were the small one erected at the tin-plate works in Newport, and one of three tons capacity at Magrins, in Wales. These apparently insignificant plants did such good work, and gave such a notable product, that the attention of American iron masters was called to the subject. In the summer of 1883 Messrs. Witherow and Oliver, of Pittsburg, made a visit to Wales, and, after a personal inspection of the process, secured the patents for the United States. In the fall of that year they began the construction of an experimental plant at Pittsburg. Like all pioneers, they had to meet and overcome a great many difficulties. Theirs was principally in obtaining proper materials for the lining of the converter, and a number of failures in this direction caused much delay. It is, in truth, only at the present time that their plant can be said to be in full working operation. They have, however, made several thousand tons of most excellent steel. The process having now passed from the experimental into the practical stage, definite statements in regard to its efficiency can properly be made. The *rationale* of the process has been carefully studied by the well known Bessemer engineer, Captain Hunt. The results of his investigation have been surprising even to those well informed in regard to the reactions that take place in a Bessemer converter.

The plant erected at Pittsburg had a capacity of three tons, and was, as far as possible, an exact copy of that used in Wales. Experience, however, soon induced several changes, such as the substitution of ganister for firebrick in the lining, and the use of movable bottoms, in order to facilitate the necessary repairs. In all this it will be noticed that no provision is made for special chemical action other than such as will result from the mechanical construction. The metal produced is properly a soft iron rather than a steel, for the per cent. of carbon is very small, in some cases but eight hundredths of one per cent. The product has been subjected to a thorough examination, both mechanical and chemical. Its composition seemed at first oddly at variance with its qualities. A sample which contained *one-half per cent. of phosphorus* withstood a tensile strain of 70,000 pounds per square inch, with 25 per cent. elongation and 36 per cent. reduction of area; and a rod made of the same material was bent into the shape of a letter S without showing the slightest fracture. These results appear very remarkable, as phosphorus has always been considered the particular arch enemy of the iron master, the malevolent spirit which danced continual attendance upon him, making his mine well nigh worthless, his manufactured product brittle, his very hair gray and dead. But now it seems that the element has been sadly slandered, and that in reality it is not an evil constituent, if kept in the right company. In the new process it is rendered harmless by the almost total elimination of both carbon and silicon. It was no secret in the laboratories ten or twelve years ago that the brittleness or cold-shortness of phosphoric irons was a result of a certain incompatibility, we might say, between this element and carbon and silicon, rather than of any inherent quality in the phosphorus itself; but since irons could be made without this element, and the former ones were always present, phosphorus continued to remain the scapegoat of the metalloids; for upon its head were heaped all the sins possible in an iron, and, like the ancient goat, it was allowed to escape to the wilderness, or rather, when its presence was known or suspected, to remain there. The Bessemer process avoided the question; it received only the purest pig irons into its exclusive society. The basic processes, on the other hand, courted the unwelcome element—the more the merrier—and by presenting to it the stronger affinities of lime and magnesia, expelled it as a phosphatic slag. The Clapp-Griffiths is the first process to make a good steel—for so the product is called, in spite of the small amount of carbon present—from the ordinary grades of pig iron without making particular provision for getting rid of the phosphorus. It is from this possibility that its greatest victory will result; for Bessemer pig is expensive and basic dephosphorization is expensive; but this process is cheap, and its product, as we have seen, is comparable with the best. In the information thus far published in regard to the process, this distinctive feature has been dwelt upon particularly, for it promises to revolutionize both the traditions and practices of iron men.

It is hardly extravagant to fancy, with this product permitting a half per cent. of phosphorus, and the Gilchrist-Thomas process calling for more worlds to conquer in the shape of saturated phosphoric ores, that the time is almost at hand when the mine owner—bless his soul for honesty!—will triumphantly assert, "We claim such and such an amount of phosphorus," instead of stating as formerly, with a doubtful air of authenticity, and a nervous twitch around the corners of the mouth, "Our ores contain no phosphorus; or, ah! only a trace." The Bessemer plants in America, though capable of such an enormous annual output, are not numerous. When one has named Troy, Bethlehem, Harrisburg, Johnstown, Pittsburg, Chicago, and Pueblo, the list has been almost exhausted, for it is an industry which must invariably yield to the concentrating tendency, so large are the operations and so great the capital employed. The plant of the Clapp-Griffiths process, on the contrary, is comparatively small and inexpensive. It is expected that its application will be chiefly in connection with the blast furnaces of the country, where the iron could be run directly into the converter, and at an expense of only about \$4 per ton could be brought into the market as steel ingots; a product, at the present prices, of fully double the value of the pig iron. At the experimental plant in Pittsburg, the cost of conversion into steel was \$6.50 per ton, but when operating on a regular working scale, with the improvements already introduced, Mr. Witherow states that he is confident of much better results; he estimates that the cost will be about \$6 per ton at mills, and from \$3 to \$4 at furnaces. The cost of converting a ton of pig metal into muckbar at Pittsburg is \$12.50, so that in this respect the new process compares very favorably. The gentlemen interested in the process have modestly refrained from sounding the death knell of the oft doomed puddling furnace, since its funeral dirge, long since composed, is still unsung on account of the uncommon activity of the "remains." To make no positive assertions, it does seem probable, however, that the terrible toil of the sweating, half-naked puddler is to be superseded by something at once more effective and more humane.

HANFORD HENDERSON.

THE POSSIBLE SUSPENSION OF OLD AGE.

DR. S. W. CALDWELL thus writes in the *Mississippi Valley Medical Monthly:*

In bygone times those profound mysteries and metaphysicians, the Rosicrucians, and still later the Alchemists, claimed to have discovered the elixir of life. They asserted that old age might be retarded and life considerably prolonged by means of an elixir, preventing or rather suspending physical decay. The celebrated Rosicrucian, Dr. Hood, whose writings became famous, is said to have reached a hundred years.

Modern science has recently made more startling discoveries than were those dreamed of by the alchemist. The possibility of prolonging life has throughout all ages been deemed worthy of notice by great thinkers, among whose number the illustrious Bacon and Hufland are enrolled. In the following remarks I shall endeavor to give the latest scientific knowledge relative to this interesting and, in some respects, novel subject.

Premature age, as engendered by various mental and physical causes, excesses, etc., does not come within the scope of this short paper. The principal characteristics of old age, as demonstrated by anatomical research, are a deposition of fibrinous, gelatinous, and earthy material in the system. Every organ of the body, during old age, is especially prone to ossific deposits. The earthy deposits have been found to consist primarily of phosphates and carbonates of limes combined with other calcareous salts.

According to the researches of Dr. Williams, of England, man begins in a gelatinous and ends in an osseous or bony condition. From the cradle to the grave a gradual process of ossification is undoubtedly present; but after passing middle age the ossific tendency becomes more markedly developed until it finally ushers in senile decrepitude. These earthy deposits during old age materially interfere with the due performance of function by the organs; hence we find imperfect circulation in the aged; the heart gradually becomes ossified, the large blood vessels blocked up with calcareous matter, and nutrition hindered.

A distinguished physiologist says: "If repair was always equal to waste, life would only terminate by accident." And it is the opinion of eminent scientists that the majority of all who pass sixty-five years suffer more or less from these ossific deposits. Therefore, bearing these facts in mind, we plainly see that the real change which produces old age is in truth nothing more or less than a slow but steady accumulation of calcareous matter throughout the system; and it is owing to these deposits that the structure of every organ is altered, thus elasticity giving way to senile rigidity. Blockage of various organs thus commenced, and sooner or later a vital part becomes involved, and death of necessity follows. The idea that old age was brought about simply, or at all, by a decline of the vital principle has long since been discarded by scientists, and the true cause found to be that of gradual disintegration of the tissues because of the inadequate supply of blood.

Again, quoting from Dr. Williams, this process is believed to be of a chemical nature, consisting in the concretion and accumulation of calcareous salts, phosphates, and carbonates of lime. The causes of old age therefore being nothing more nor less than ossific deposits, let us for a moment look for the causes and influences leading to the condition we have described; for having arrived at the predisposing causes of senile decay, it yet remains for us to go still further, and seek out their origin.

The two principal causes of old age are, first, fibrinous and gelatinous substances; and, second, calcareous deposits. According to recent researches of Delany Evins, the origin of the first, the fibrinous and gelatinous, may undoubtedly be traced to the destruction of atmospheric oxygen, and demonstrable by the following argument:

In the air we breathe the relative proportion of oxygen to nitrogen is 22 to 78; and although oxygen is in far smaller bulk, yet it is the most active element. Now, oxygen has an affinity for every other element except fluorine; and as oxygen plays by far the most important part in these chemical changes constantly at work in the economy, life itself is but a constant waste by oxidation and reparation by food. In the blood exists albumen and fibrin, themselves resolved into component parts, carbon, hydrogen, nitrogen, oxygen, sulphur, and phosphorus. Fibrin, it is claimed, contains one and five-tenths more oxygen than albumen. Now, oxygen converts albumen into fibrin, fibrin itself being but an oxide of albumen. Although unquestionably fibrin nourishes the organs of our body, yet it becomes at times, as we reach the cool and shady walks in the evening of life, accumulated in redundant quantity, blockading the streams of life as doth the chilling winds of winter the mountain rivulets. There is always a struggle going on in our bodies between accumulation and elimination, and thus it is that the fibrinous and gelatinous accumulations of old age are chiefly traceable to chemical action of atmospheric oxygen.

The calcareous deposits next claim our attention, being proved by anatomical investigation to be peculiarly characteristic of old age. In the human body water forms seventy per cent. of its aggregate weight; in fact, there is not a single tissue but contains water as a necessary ingredient. Now, water holds certain salts in solution, which become more or less deposited, notwithstanding the large proportion eliminated through the secretions. Nevertheless, it is but a question of time before these minute particles deposited by the blood have a marked effect in causing the stiffness and aridity of advanced life. The reason why in early life the deposit of earthy matter or salts is so infinitesimal is simply because they have not had time to accumulate. Besides providing the requisite elements of nutrition, food contains calcareous salts, which upon being deposited in the arteries, veins, and capillaries, become the proximate cause of ossification and old age. Mr. Lewis, an eminent English scientist, in his Physiology of Common Life, says: "Moreover, in food we are constantly introducing different substances which produce variation in the nutrition of the parts. These different accumulations exert their influence in those changes named age, and they culminate in the final change named death."

Having now traced the primary existence of calcareous matter to food itself, it is consequently a subject of no small moment to ascertain the various dietetic articles containing these salts. As a matter of fact, everything we eat does contain them to a greater or less degree. The cereals are found most rich in them; so bread itself, the so-called staff of life, except in great moderation, most assuredly favors the deposition of these salts in the system. The more nitrogenous our food, the greater its percentage of calcareous matter; thus a diet composed principally of fruit, from its lack of nitrogen, is best adapted for preventing or suspending ossification.

Moderation in eating must ever be of great value as an

agent for retarding the advent of senile decay. Large eaters more rapidly bring on ossific deposits by taking in more than is utilized or excreted, naturally resulting in blockading the vessels and destroying their normal functions. According to the best authority I have been able to consult, the following seem to be the best articles of food, as containing the least of earthy salts: Fruit, fish, poultry, flesh of young mutton and beef, because, as before stated, of their being less nitrogenous. Fluids, as part of the diet, are of special import. All well and spring water contains considerable of the earthy salts, and should therefore be avoided, and cistern water used in its stead, because water is the most universal solvent known. Therefore, if when taken into the system clear of foreign matter, it is to that extent the better prepared to dissolve and take up those earthy salts, and convey them out of the system. The addition of fifteen or twenty drops of dilute phosphoric acid to the glass of water, and drank three times a day, will add to the solubility of these earthy salts.

THE TROCHOIDED PLANE.*

By LAWRENCE HARGRAVE.

I HAVE been told that the subject of this paper is one that would interest the members of this Society, and therefore I have strung together my thoughts, ex-

FIG. I.

periments, and deductions that refer in any way to the trochoided plane, pointing out where I see Nature working with it, and how it can be used by man for the transmission of force; and I trust that if other members have heard of or made similar observations, they will bring them forward, so that my mistakes may be corrected by comparison with the ideas of others, and also that the truth may be elicited about a matter that does not seem to get its fair share of investigation.

I will first endeavor to make clear what I mean by the several terms I use, or have had to invent, in describing the action of the trochoided plane.

The "*trochoided plane*" is a flat surface, the center of which moves at a uniform speed in a circle, the plane being kept normal to the surface of a trochoidal wave, having a period equal to the time occupied by the center of the plane in completing one revolution.

By "*normal*" is meant tangential to an undulating surface.

"*Orbit*" is the path of any particle of a substance through which undulations are being passed.

"*Crank*" is the radius or radius-vector of the orbit.

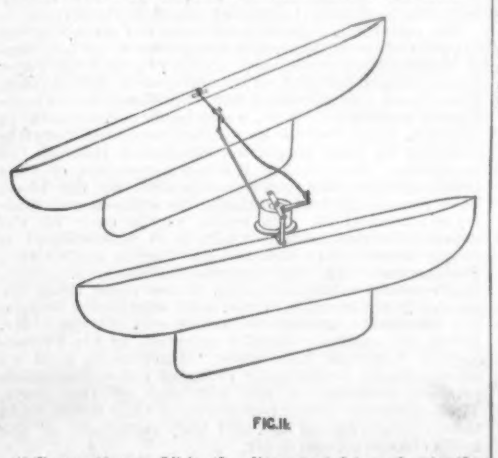


FIG. II.

"*Connecting-rod*" is the line at right angles to the trochoided plane; the length of the connecting-rod is equal to the crank multiplied by the secant of the pitch-angle. I used to call this the normal to the trochoid, but to avoid confusion I shall in future call it the connecting-rod, unless some one points out its true mathematical designation; radius-vector of the trochoid seems a good name also, but not so descriptive as connecting-rod; every one knows what a connecting-rod of a reciprocating engine is, and its familiar motion.

"*Pitch*," or length of wave, is the distance of waves from crest to crest, measured in the line of propagation; the length of a trochoidal wave is equal to the length of the orbit of a particle divided by the co-tangent of the pitch-angle.

"*Pitch-angle*" is the angle contained between the crank and connecting-rod, when the trochoided plane is at half the height of the wave; it is also the angle contained between the trochoided plane and the guides when in the before-mentioned position.

By "*the guides*," I mean that straight line drawn by the end of the connecting-rod farthest from the trochoided plane during the passage of half a wave.

The "*trochoidal wave*" may be defined as the projection of a right helix on to a plane parallel to its axis, and is resolvable into an infinite number of trochoided planes.

The "*prolate-cycloidal*," "*cycloidal*," and "*curtate-cycloidal*" waves are the projections of the helix on to a plane that is at various angles with the axis of the

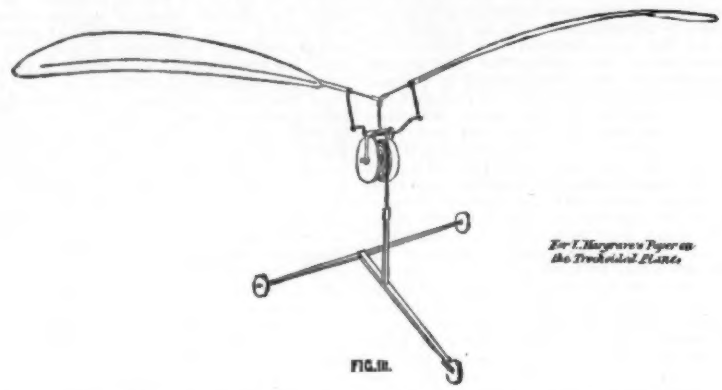


FIG. III.

helix. If we take a right cylinder, and cut it diagonally, and open the two cylindrical parts out flat, we get two trochoidal waves or curves of sines.

If one circle touch another internally, and we cut through the circles at the point of contact, and open out the figure till the circumference of the small circle becomes a straight line, the resulting figure will be bounded on the other side by a curve of the same class.

The line drawn on a uniformly moving sheet of paper by a pendulum swinging at right angles to the line of motion of the paper is also the trochoidal wave.

If the waves are prolate-cycloidal, the orbits of the particles are elliptic, and the crank or radius-vector follows Kepler's second law, describing equal areas in equal times, the focal distance being measured from the crest of the wave; the connecting-rod varying in length from the focal distance to infinity, and it is obvious that when the distance of the foci becomes O, the waves are trochoidal and the orbit is a circle.

It is evident that if the weight of the moving parts be neglected, it is immaterial at which end of the connecting-rod the trochoided plane is placed, and that waves may be thrown or generated by a plane, or infinite series of planes, that are trochoided in unison; and that each plane may be trochoided by moving the ends of the connecting-rod or its continuations in the various combinations of the straight line, circle, and ellipse, and doubtless other figures; but each combination is reducible to the simple principle of the plane at right angles to the connecting-rod, moving in a circle, and guided by a straight line.

The area of the triangle that is bounded on two sides by the crank and connecting-rod is directly proportional to the thrust at right angles to the guides; and the thrust is greatest at the center, and decreases gradually to the sides of the column of matter acted on by the trochoided plane, so that there is no violent disruption of any two parallel streams.

If I have succeeded in communicating my views with regard to the motion of a plane surface when acted on by an undulating one, and the converse, it will be obvious that if the undulating surface is rolled up into a cylinder, with the axis parallel to the direction of propagation of the waves, the same reasoning will hold good, and reduce the examples of cylindrical waves to plane waves; but when we consider the action of the particles composing the axis of the cylinder, it becomes necessary to explain the spherical wave.

Let us suppose a spherical shell to be composed of any elastic medium, also the polar axis to be similarly constituted; let the point of bisection of the polar axis come exactly between two of the particles composing it, which two particles we will set approaching and

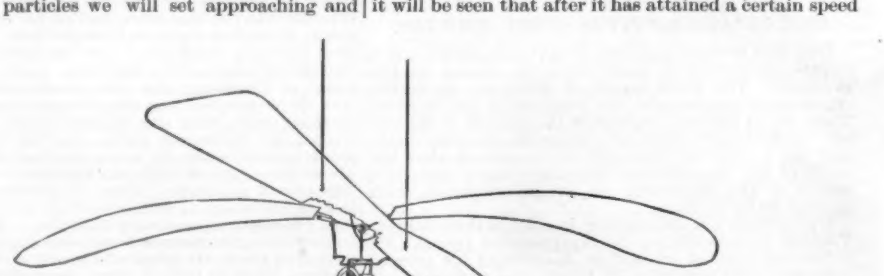


FIG. V.

receding from one another; this will send an equal series of waves of extension and compression through the two halves of the polar axis, culminating in the pushing out and pulling in of the poles synchronously, and the generation of a series of ring waves passing over each hemisphere, meeting at the equator, and crossing each other to the opposite poles. The orbits of the particles composing the sphere will at first be long ellipses; as the waves recede further from the poles, the orbits become circular; one wave length from the equator they become elliptic again; at the equatorial plane they move radially to the sphere, and after one wave has reached the opposite pole, every particle of the spherical shell will be moving radially and harmonically with the two central vibrating particles. Any number of atoms may be conceived as being at the center, and completely filling the sphere, vibrating in every direction, causing any number of spherical waves on the spherical shell, crossing in every direction.

If we take the converse of this, and suppose the equatorial particles, or those in any parallel of latitude, to be set vibrating in unison, radially, the resultant will be an intensified vibration of the polar axis longitudinally; this is closely analogous to a sea wave meeting a vertical obstruction, and causing that horizontal downward-moving vortex that is so destructive to the foundations of steep smooth marine structures *in shallow water*. **Where this occurs, the practice is to tumble in loose blocks of stone or concrete, the effect of which is to break up the vortex, and rob it of its power.**

Our Sydney summer thunder-storms often show this atmospherically; the sun's heat over the land causes the sea-breeze to come in from the N. E. with increasing force as the day advances, and the heated air returns to sea by an upper current from the S. W. There is an upward current at the Blue Mountains, or where the sea-breeze takes off in force, and heavy cumulus clouds gather, their tops being drawn out by the return current in long streaks pointing seaward; toward evening, if more air comes in than can readily be restored by the upper current, a plenum is formed, resulting in an uprush, sudden cooling, and downfall of air at the center of the disturbed area, that produces a horizontal vortex, which pushes itself under the sea-breeze, and shows its presence by a long roll-shaped cloud, rotating on its axis, the upper surface moving from N. E. to S. W., the under surface almost touching the tree tops, moving from S. W. to N. E. Sometimes the thunderstorm comes without this characteristic cloud, some-

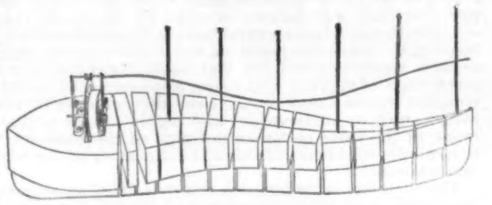


FIG. IV.

times there are two of them; there is a similar roll-cloud often seen on the pampas of South America, described, I think, by Dr. Gould. I believe I am correct in saying he does not account for its formation in this manner. If we substitute the trade winds, equatorial calms, and return trade winds for the sea-breeze, plenum, and return S. W. upper current, the result is one or more hurricanes or cyclones instead of the roll-cloud.

Allied phenomena are those witnessed when a drop of water falls into water; if the drop falls from a short distance, only ring waves are formed; if from a height, the drop seems to make a hole in the water, as well as the ring waves, and the closing up of this hole sends up a peak with sufficient force to detach another drop from its point.

Also, if a large drop be allowed to fall from a height, it will be seen that after it has attained a certain speed it will leave its spherical shape and spread out into an irregular ring; the movements of the particles of the ring being similar to those composing a smoke ring.

But to return to the spherical wave; if one of the central particles bears an infinite proportion to the other particle, the vibration of the smaller one will send waves through a sphere surrounding both, resulting in an equal vibration of the antipodes of the smaller particle.

When this action of spherical waves is applied to the supposed string of spherical particles composing the axis of a cylindrical wave, it is obvious that we may conceive the great circles passing through the spheres

* Read before the Royal Society of N. S. W., 6 August, 1884.

and the longitudinal axis of the string of spherical particles, as being shoved out of shape or made elliptical, first transversely, and then longitudinally; stated otherwise, the pulsation of the spherical particles is their vibration or change of form from the ellipsoid to the oblate-spheroid; here, I take it, we are brought face to face with one form of infinity.

From this it is evident that if the ultimate composition of a cylinder be elastic spherical atoms, the organized pulsation of these atoms in unison will produce waves on the surface of the cylinder, and the converse proposition will also be true, that is, if the pulsations be produced in a pipe covering the cylinder, the waves will be communicated from the inside of the pipe to the contained matter.

I will now state my views as to the formation of vortices by an imperfect plane passing end on through a viscous medium.

If the plane is perfect and of no thickness, and the medium homogeneous, no vortex can be formed; but if the plane has imperfections, at approximately equal distances, zones of compression and tenuity will be formed at and between the imperfections, and their amplitude will constantly increase as they recede from the leading end of the plane, the stratification of the medium parallel to the plane becoming prolate-cycloidal. When the proportion between the amplitude of the disturbances and the wave length or distance apart of two zones of compression exceeds that of one to two, or the prolate-cycloid passes into a curtate-cycloid, vortices will be formed in the loops, and go on increasing in diameter till they nearly equal the wave length, when anti-vortices will be formed between them that either break up the system of waves, or begin a fresh series farther out from the plane. If the force acts at right angles to the plane, a vortex is generated behind the plane

A breaker shows the vortex initiated vertically, but gravity prevents its complete formation.

A common instance of this action is seen in the skin of eddying water that adheres to the side of a vessel in motion, and it is my opinion that vortices formed in this way eat away the tips of screw-propeller blades in the unaccountable manner we so often see them. I may also add that the pitting of the interior of steam-boilers at and near the water-line is to my mind clearly the mechanical action of vortices formed by the rapid circulation of the water.

This brings us to a considerable distance from where I started with the plane wave, but I thought it best to indicate the natural sequence and deductions that follow from the trochoided plane to the vortex, and will now try to show some natural movements of matter and mechanisms that I associate with the different sorts of waves.

First, about ocean waves, we find much has been written by the late Mr. Scott Russell and others, dealing with their form, and the motion of the particles composing them, about the forced wave and the free wave; but no one, as far as I have read, seems to note, when dealing with the trochoidal form, the motion of the imaginary line that I call the connecting-rod, and which appears to me to be as important in describing that wave as the radius is to the circle (this is probably due to the form of long free ocean waves being *approximately* trochoidal); and I do not hesitate in saying that the connection of the trochoidal wave and trochoided plane with our simplest mechanical movement, the crank and connecting-rod, opens up a field for the development of engineering talent as extensive as the discovery of the screw did when our mechanisms were limited to the use of the lever and wedge.

The power may be abstracted from the swell of the ocean by means of the trochoided plane, thus: take a flat float, and rigidly connect a plane at some distance below parallel to the float, and it will be found that the plane and float alternately pull each other in the direction of propagation of the waves, the result being that the apparatus progresses through the water faster than a float without the plane attached.

If the plane is fixed vertically, or at right angles with the float, the resultant is in a direction contrary to that in which the waves are moving.

Again, if two floats are connected by linkwork end to end, and separated by a distance equal to half a wave, the trochoidal movement of each float may be used to propel the whole concern.

From this it follows that when a vessel has another in tow in a seaway, the length of the hawser should be such that both vessels are as near as possible to the crests of waves at the same time; if there is a cross sea, this is impossible; if the sea is at all regular, attention to this, whether by accident or design, saves many a savage jerk to the tow-line; the distances, ½ wave, 1½ waves, 2½ waves, are obviously those to be most avoided.

I will now draw your attention to the motion of living organisms, and how their movements seem to me to have a common origin, and that the trochoided plane is the mechanical power almost universally used by Nature for the transmission of force.

The backfin of a fish is a good example to begin with; observation will show that the ends of the spines that keep the membrane extended are points in the curve of sines, or trochoidal waves, and that if they are rotated on their axes in unison, or oscillated from side to side harmonically, a series of complementary waves will be thrown by the membrane toward the tail; the cross-section in the first case is an isosceles triangle, and in the second a sector of a circle; the membrane of the tail is trochoided similarly, so as to raise or depress the head; the prolate-cycloidal wave is also used in fin swimming. This action of fins is best seen when fishes are confined in a bowl, and appears to me to be only used when the speed required does not necessitate the use of the large muscles of the body; the converse of this proposition is seen when the wind makes trochoidal waves on a fluttering flag.

The geometry of body swimming may be seen in its simplest form in a slow-moving organism like the leech. You will observe that the head is raised and thrust forward, depressed, and drawn backward, describing a circle for each undulation that is passed through its frame; as the motion increases in rapidity, the circle becomes an ellipse, and then a straight line. Then we want to know how the undulations are produced in its frame; here the cylindrical wave comes to our aid, and it becomes evident that if two cylindrical muscles be inclosed in a skin (take the swimming of the eel as the type), and a series of waves be passed through each muscle, such that the thick part of one wave is abreast the thin part of the wave on the other side of the eel, the backbone and skin will necessarily take a trochoidal form, and as long as the waves are generated the eel must go ahead.

It will be observed that if the fish or eel is swimming with its body, and the fins on the back and belly are kept rigidly extended, they serve to increase the trochoided surface. The body swimming of fishes reaches its extreme form in the sunfish, whose powerful body cuts trochoids of extreme length in proportion to their amplitude.

Porpoises, when rolling, seem to cut vertical prolate-cycloidal waves, and they blow when passing the crests; the horizontal position of the tail is well adapted for this mode of progression.

If the top edge of the tail of a schnapper, or other deep fish, be twisted to one side, and the lower edge to the other side, and the trochoidal action of the body continued, the fish at once turns on its side, and will thus be able to dive suddenly.

The effect on the water of this action of fishes that are long enough to contain a number of waves in their length is a tendency to produce right-handed vortices on one side, and left-handed ones on the other, so that, after the fish has passed, the two series gear together, as it were, like a train of equal sized cog-wheels.

If we conceive the action of the backfin, as thus described, to be communicated to a series of legs on each side, as in the centipede, the effect will obviously be progression along a surface; and if we cut off all the legs but two pairs, separated by a distance equal to one wave length, we have the quadrupedal action popularly assigned to the giraffe; if the two pairs are only half a wave length apart, we have the trotting pace of a horse, and the various other paces become clearly dependent on the length of the wave used by the animal. The legs and body of an alligator or lizard show the connection between many-legged and four-legged progression, as well as any one instance I can point to; but it is impossible to define any hard and fast line between any two classes, as the more instances of progression we notice, the more it is forced upon us that they are but links in a chain, the two end links of which are unknown, while any two adjoining links are hardly distinguishable. The swinging of the hands and arms in walking or running is evidence that bipedal is evolved from quadrupedal progression, which to me seems to have developed from the trochoidal action of a fin.

When the amplitude of the waves is in a vertical plane, each pair of legs is moved together, and the form of the wave is plainly seen in the back of a dog when going full split; this method of progression reaches its extreme form in the hopping birds.

Slugs, and other so-called one-footed organisms, move by a beautiful application of the trochoided plane; their mode of progression will perhaps be best understood by examining the converse movement, as exemplified in the fluttering of a flag, in which case, as I pointed out before, the passing wind communicates ripples to a stationary flexible plane; in the slug the ripples on its base are applied to the surface across which it wishes to move; the crests of the ripples are transversal to the longitudinal axis, and move toward the tail; the slime is a great aid in passing over a smooth surface, as a partial vacuum can be formed in the hollow of each wave; if the surface of the slug's foot is covered with cilia, the undulatory motion of these may be conversely seen on a field of waving corn as the breeze passes over it; the mode of progression of some caterpillars is parallel with that of the slugs.

In order to make this more than mere theory, it became necessary to pursue one of two courses: either to go in for an elaborate system of instantaneous photography in connection with a chronograph, or to make some models, the geometrical construction of which would show the trochoided plane, and the outward appearance and movement of the apparatus would appeal direct to the eye. I have adopted the latter course, for several reasons, the principal one being that the first method, besides being very expensive to me, would only be accessible, if understood at all, by the few; while the second course is now within the reach of any boy who can handle a few tools. As these are experiments that I venture to call capital, it will be excusable if the details seem rather trivial.

The simplest trochoided plane may be constructed by attaching a flat surface at right angles to the connecting-rod of the ordinary crank and connecting-rod motion of the reciprocating engine; and if two of these be coupled with the cranks at right angles (Fig. 1), the sum of the sectional areas of the columns of wind or water, acted on by the two planes, will be the same at every point in one revolution; and if the apparatus be placed so that wind or water act on the planes at right angles to the guides, a uniform rotary motion will be communicated to the crank-shaft; if we rotate the machine by steam or hand, motion is communicated to the air or water by the two planes.

If two oar-like planes (Fig. 6) be protruded vertically under the counter of a vessel, and trochoided through ball and socket joints or cross-journals, the connecting-rods and guides being inboard (as was shown in the largest model), it will be found to possess several advantages as a propeller that are not to be despised; it seems impossible to foul it, as no amount of floating canvas, nets, or ropes could be twisted round the blades; the blades would be no impediment to sailing, and a rudder would be superfluous, as the steering is effected by rotating the guides; the engine would not

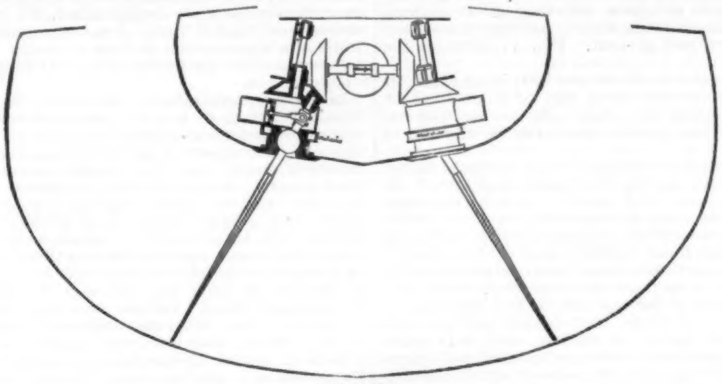


FIG. VI.

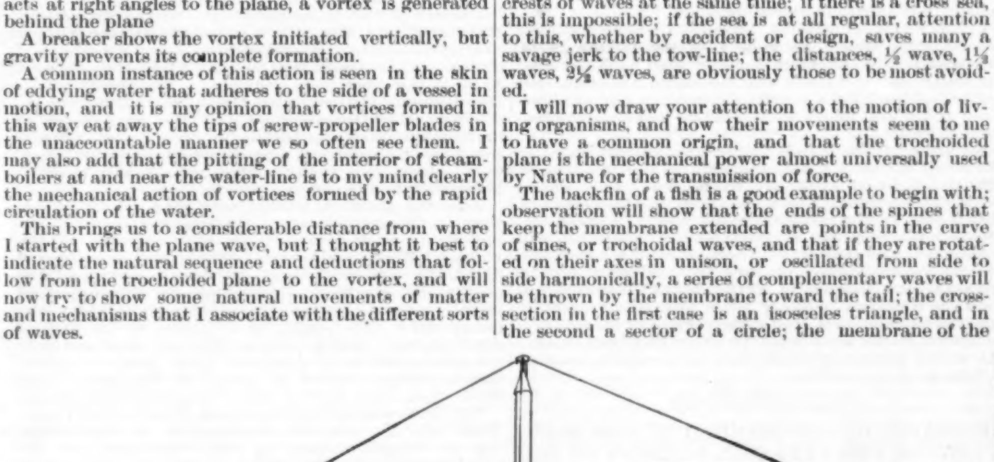


FIG. VII.

THE TROCHOIDED PLANE.

need reversing gear, and the pitch could be made easily adjustable by altering the length of the connecting-rods of the propellers.

Again, if we take two floats that offer equal lateral resistance (Fig. 2), and fix a bar in the center of each float at right angles to the vertical longitudinal section, then put a rotating crank with the shaft vertical in the center of one of the floats, and a guide of some description on the bar of the other float, unite the end of the bar from the crank-float to the guide, and the end of the bar of the float that carries the guide to the crank-pin, and it will be seen that the whole apparatus will be propelled through the water by rotating the crank; this is like a common feat with skates on ice. Two wheels may be substituted for each plane in this model for motion on a surface, but the results are unsatisfactory.

If a pair of equal floats be made with a total displacement exceeding that of a man, it will be found that crank, guides, and connecting-rod can be dispensed with, and that the floats can be trochoided by the feet. The steering is effected by bearing a little heavier on the float toward which it is wished to turn; the increased skin resistance will do the rest. The total absence of mechanism will commend this form of exercise, and I hope to see it become a feature in our regattas.

But these experiments are not calculated to convey to every eye the identity of the trochoided plane with the mechanical power used by a fish in swimming, so it was thought necessary to make something with a general likeness to a fish, and cut it up into a number of sections, and unite the sections again so that they were free to move from side to side on vertical hinges (Fig. 4). Each section was provided underneath with a keel, and every alternate section had a vertical guide stuck in its center; the section corresponding to the head of the fish was enlarged so that it was able to float a coiled spring driving a wheel and pinion; on the end of the pinion shaft was soldered a right helical wire; the diameter of the helix was made equal to the amplitude of the trochoidal waves it was intended the model should use, and the pitch was made equal to the wave length. The forward end of the helix was brought into the center, as if it had been twisted round a spindle instead of a cylinder; this helical shaft was rove through the guides on the alternate sections of the model, and, as you saw, trochoided all the planes together, and made the model swim in a strikingly natural manner; by drawing the model tail first through the water, the operation is reversed, and the trochoided planes wind up the spring. This model is remarkable for the diminutive nature of the motive power, the easy trochoiding of the planes, and the small percentage of slip.

Here I would remark that a running stream offers a good field for investigation on this subject; however straight its channel is cut, it will, if left to itself, meander, and the bends work down stream. This may be best seen when the stream runs through an alluvial flat; it will be observed that the down stream sides of the bends are continually being washed away, and that deposits are made on the down stream sides of the points.

If the upper ends of the guides in the last-described model be connected to a rigid bar, the motion of the helical wire will make the guides, or an elastic web covering them, take the form of a fin, showing the action I have previously described.

Again, if a number of pieces of wood be rove loosely across a rotating helical wire, the ends of the pieces will be trochoided like the legs of a centipede; owing to difficulties about making the clawing apparatus, this was made to float in water, and you had an opportunity of judging for yourselves whether or not it proved the truth of my deductions.*

I will now direct your attention to the swimming of that common jelly-fish (*medusa*), as being a slow and easily observed case of cylindrical waves; watch closely the movement of the equatorial ring of its hemispherical head, and the path described will be seen to be similar to that of a zone of particles in a smoke ring; the equatorial ring moving forward when most contracted, and backward when most expanded; the superficial resultant being annular or cylindrical trochoidal waves thrown backward, and the motion of the jelly-fish forward.

The progress of an earth-worm through a cylindrical hole is another obvious case of cylindrical waves, and from the worm's movement on a flat surface it appears capable of throwing more than one wave toward its tail at the same time, or, in other words, that its body is longer than a wave length; in this case the motion of the rings is evidently not circular, as in a smoke ring, but very elliptical, with the major axis parallel to the direction of propagation of the waves.

If you note the mechanical action of swallowing, it will be evident that it is the converse of the motion of the worm; observe the jaws or mouth open and thrust forward, closed and drawn back for a fresh bite, the prey being forced to the stomach by a similar movement of the rings of the gullet.

The trochoidal action of fins, muscles, and legs seemed so plain that I could not help being led to theorize on the action of wings in flight; I say theorize, simply because I have not a flying-machine to show you, but the chain of evidence seems so complete that I have no doubt it will soon be accomplished without the aid of the screw or gas bag.

The wings of flying-fish are, in my opinion, only used for flight, or when the fish is swimming very slowly with its fins alone, without trochoiding its body.

There is a distinction between the vertical lift we see when a bird hovers or rises straight up from the ground, as exemplified by skylarks, hawks, partridges, and the horizontal flight of ducks, pelicans, and albatrosses; in the first case, the wing, which is in effect a plane, is rotated in a cone, and kept normal to a trochoid during each revolution; the connecting-rod is moved in a plane at right angles to the axis of the cone, and the guides are horizontal, the plane of the wing being in the line of the connecting-rod, and not at right angles to it; the path cut in the air by this motion is a zigzag, one of the pieces between two angles being half a trochoid, the two half trochoids making up each revolution of the axis of the wing.

In horizontal flight the conical movement is the same, but the connecting-rod is at right angles to the plane of the wings, and flight is the resultant of gravity and the waves of air being thrust downward and backward by the wings. The part played by the plane of the body and tail in flight is the same as that of the second plane in Fig. 2. I have not put the second plane in all the models of wings, as I think it useless before the power of the machine is sufficient to overcome its specific gravity. (Fig. 5 and all subsequent models have the second plane.) Peacocks' tails and the plumes of birds of paradise are a hinderance to flight, and the effects of sexual selection.

The same action takes place in the wing that I mentioned about the leech's head, and is equivalent to sliding the plane along the connecting-rod toward the guide-pin, so that the center of effort moves first in a circle, then in an ellipse, and finally in a straight line. I have shown the first and last movements in Fig. 7 and Fig. 3.

These are the motions we often see when the passing breeze sets a blade of grass rotating; it is common with flat leaves having thin stalks, and must have been observed by every one present. Fig. 7 will show the geometry.

These remarks refer to all wings, but, in addition, it is observable that jointed wings can be trochoided by opening and shutting the wing, the connecting-rod working in a vertical plane transversal to the line of flight.

A little consideration will show that turning, rising, and descending are merely resultants dependent on the position of the center of gravity, and the direction the waves are thrown in; by depressing one side of the tail, and raising the other, a portion of the thrust is directed to one side; and in the construction of a flying-machine, it is an unnecessary waste of power to try to lift the enormous rudders that are given such prominence to in many of the schemes we see depicted.

As to the soaring of birds, that branch of flight has been well argued lately in *Nature*, and it is quite clear to me that the birds work the upward and downward currents of the air, the existence of which is plainly shown by the form of clouds and smoke.

Natural selection and the survival of the fittest account for the development of the wing membrane on the afterside, and its curtailment on the leading edge of wings, and not as I have made it in the models equal on each side of the quill: it is so constructed because I recognize the course the evolution of artificial flight will take, and that in its first stages the motion will be slow; and, as we gain confidence in the construction of the machine, we shall notice that by leaning our body in the direction we wish to go, that is, altering the position of the center of gravity, and reducing the speed of the planes, our object will be attained; and then we shall find the leading edge of the planes will be liable to double up and get damaged, making it necessary to follow in the footsteps of Nature.

These are my views, stated as concisely as I can; and if you think there is any novel truth embodied in them, this Society is welcome to any of the laboratory models that aided me in finding it out.

In conclusion, gentlemen, I should like your opinion as to *whether or not* there is evidence to show that there is a power almost universally used by Nature for the transmission of force, that can claim to be regarded as distinct from those previously used in our mechanisms; and if not, under what head do you class the trochoided plane?

Regarding these models,* they are exhibited here as the result of about a dozen efforts in the direction of artificial flight, and they are, to the best of my knowledge, quite original; and let me point out the certainty that if only twenty more are made, several marked improvements must be evolved; and if there are any mechanical members of this Society who see that the successful construction of such a machine would be advantageous, they could, at the cost of a few shillings, copy these models, making such alterations as their imperfections suggest, and a comparison of the improved models would show what progress had been made; this would hasten a process that is, to say the least of it, laborious and tedious.

* A six-legged model was afterward made and exhibited that worked satisfactorily.

* These models were somewhat similar to Figs. 3, 5, and 7, and were not shown at the previous conversazione.

ABSTRACTS OF PAPERS READ AT THE MEETING OF THE NATIONAL ACADEMY OF SCIENCES, WASHINGTON, APRIL 21-24.

IN addition to the brief sketch presented in the SCIENTIFIC AMERICAN, we subjoin the following abstracts of papers presented at this meeting, giving all that were of any other than purely technical value.

The meeting of the Academy was fully attended. On the last day, thirty-four members answered the roll call, being the largest attendance ever recorded.

Besides the scientific papers, biographical sketches were read of Dr. J. J. Woodward, by Surgeon-General Billings, of Gen. A. A. Humphreys, by Gen. H. L. Abbot, and of William Stimpson, by Prof. Theodore Gill.

Winged Insects from a Paleontological Point of View. By Samuel H. Scudder.—The division of hexapod insects into orders has undergone no very striking changes since the time of Linne and Fabricius, the founders of entomological science; new elements, indeed, have entered into their definitions, but the main divisions introduced by these pioneers have, on the whole, stood the test of time and increasing knowledge in a somewhat remarkable way. Unquestionably this is due in large measure to a somewhat unusually sharp delimitation of most of the main groups, recognized even by the least observant, who, if given a thousand chance insects from his own neighborhood, would be pretty sure to separate from one another the wasps, the moths, the flies, the beetles, etc., or at least most of them. There are of course a few forms (few compared to the mass) which would prove disturbing elements, and there are some concerning which the best informed are not wholly agreed, there are also some groups about whose taxonomic value there is still disagreement such as whether the Heteroptera and Homoptera should be looked upon as orders or as primary divisions of the order Hemiptera; others concerning which there is some dispute whether they should be separated as orders, or as mere families of one of the long established orders, instances of which may be found in the Westwoodian orders of Aphaniptera and Euplexoptera; still others, not regarded as distinct orders, concerning whose nearest affiliation there is or has been question—as in the case of the so-called Pseudoneuroptera. This is in effect only to say that here, as in other great zoological divisions, there are aberrant groups, and the main groups themselves are unequally delimited.

The attempts, however, to group the orders into larger divisions still subordinate to the grand hexapod type have resulted in very diverse presentations, according as one or another set of organs, or other peculiarities, were deemed of prevailing weight. The two which have found the most adherents have been that which separated the mandibulate from the haustellate insects, and that which divided them from each other according as their metamorphosis is complete or incomplete. To the first, the objection naturally arises that it places the Hemiptera beside the Hymenoptera, Lepidoptera, and Diptera, rather than with the Coleoptera and Orthoptera, to which by all other points in their bodily structure and by their metamorphoses they are certainly far more closely allied. To the latter, that we find very varied forms of metamorphosis within the limits of a single order, so that it would require a dismemberment of the orders to uphold the distinction in a logical form.

In the attempts alluded to above, naturalists have simply selected, as it were, combinations of acknowledged ordinal peculiarities in order to form and distinguish their superordinal divisions, and have failed to search deeper into the general structure for more fundamental characteristics. Packard, however, has done this, and by employing the terms Metabola of Leach, in a modified sense, and Heterometabola, has brought the Hymenoptera, Lepidoptera and Diptera under the former, and the other orders under the latter. In a paper published six years ago on the Early Types of Insects, I gave my adhesion to this view, and strengthened it, as I believe, by some additional characteristics drawn from the regional divisions of the trunk. In the Metabola, the thorax, supporting the organs of aerial locomotion—primary feature of the Hexapoda as a whole—is very highly organized and compact, well differentiated from both head and abdomen, the prothorax very small; the body is generally cylindrical; the mouth parts prolonged into a beak of some sort, and the mandibles rarely opposed at tip; the frontwings are membraneous and larger, generally very much larger, than the hind pair; the larva is cylindrical, and in no way resembles the adult, and the pupa is inactive. In the Heterometabola, on the other hand, the prothorax is larger, and the joints of the thorax are less compacted, as a rule, than in the Metabola, or, if compacted, generally massively soldered to the abdomen; the body is usually flattened; the mouth parts are generally not prolonged into a beak, and the tips of the mandibles are generally opposed; the front wings are generally more or less coriaceous or with very numerous and thickened veins, and generally smaller than the hind wings; the larva is usually depressed, often resembles the adult in form (excepting, of course, in the wings), and the pupa may be active or inactive.

The exceptions, in special points, to the above general statements are not few, especially among the less homogeneous Heterometabola, but if any superordinal division of Hexapoda is to be looked for, it would seem to be on the lines here indicated. The points which are especially disturbing are the opposition of the mandibles in the Hymenoptera, and the appearance of many metabolous characteristics among the Neuroptera properly speaking, a group which is, nevertheless, as a whole, admittedly related most nearly to other heterometabolous orders.

That the Metabola should rank, as a whole, higher than the Heterometabola can scarcely be disputed; the regional division of the body, the structure of the wings for flight, and especially for strong and directed flight, the complication of the mouth parts, and the universally complete metamorphosis and quiescent pupal state, are fundamental features, in which the hexapodal type is carried, as a whole, to its highest development. And yet, as we shall see, there are some features in which its members have held to fundamental characteristics of paleozoic hexapods more firmly than have most of the heterometabolous groups.

This brings us fairly to the main object of this paper. What were the relations of the ancient to the modern types of winged insects? In what succession did the two superordinal divisions of insects appear, and at what period the different orders as we now recognize them? What light, in short, can paleontology throw upon the origin and succession of insects?

In attempting some years ago, in a paper already referred to, to answer this question in a broad way, I stated that all the orders of Heterometabola, and none of Metabola, had been found in paleozoic deposits. Today I shall have to modify this proposition. Not only have numerous discoveries been made in paleozoic deposits within the past six years, but those already known have been subjected to more rigorous study and wider comparisons, which have considerably enlarged our knowledge. Protophasma had then only just been discovered, an insect which has done more than any other, excepting Eugereon, to throw light on the fundamental characteristics of the early world of insects; and even now Brongniart has published but five or six examples of the treasures of Commentry, a place which has already yielded remains exceeding in numbers those of all the rest of the world put together. Nor must we leave out of sight his discovery of a winged insect in the Silurian.

While our knowledge of paleozoic insects is thus shown to be clearly still in its infancy, it may appear hazardous to attempt to formulate statements of a broad and sweeping character concerning the appearance of the primary groups of insects in paleozoic times, especially if I am already compelled within six years to modify such assertions then made. Yet when I point out the nature of this modification, made after a special study of every known paleozoic form, it will appear less hazardous.

The modification I would introduce is to this effect: That while we may recognize in the paleozoic rocks insects which were plainly precursors of existing Hetero metabola, viz., Orthoptera, Neuroptera (both Neuroptera proper and Pseudoneuroptera), Hemiptera (both Homoptera and Heteroptera), and perhaps Coleoptera—and no Metabola whatever—a statement almost identical with that previously made, we may yet not call these Orthoptera, Neuroptera, etc., since *ordinal features were not then differentiated*; but all paleozoic insects belonged to a single order which, enlarging its scope as

outlined by Goldenberg, we may call Palæodictyoptera; in other words, the paleozoic insect was a generalized Hexapod, or more particularly generalized Heterometabolon. Ordinal differentiation had not begun in paleozoic times.

It will be asked, Were there, then, no cockroaches in paleozoic times? I answer, yes; cockroaches, but no Orthoptera; Palæoblattariæ, not Blattariæ; that is, Palæodictyoptera, not Orthoptera. Mayflies, but they were Palephemeridæ, not Ephemeridæ—again, not Neuroptera, but Palæodictyoptera. Walking sticks, but no Phasmida—only Protophasmida, another group of Palæodictyoptera.

The grounds for this view are as follows: 1. No group of paleozoic insects has yet been studied carefully—and it is important to observe that, though our knowledge of them is of necessity fragmentary, yet the more perfectly they are known the clearer is this true—no group, I say, has been carefully studied which does not show, between it and the modern group which it most resembles, differences so great that it must be separated from that group as a whole, as one of equal taxonomic rank, as in the case of the three related groups last mentioned.

2. That the different larger groups of paleozoic times, of which we now know nine or ten, were more closely related to one another, at least in the structure of their wings (which is the only point of general structure yet open for comparison), than any one of them is to that modern group to which it is most allied, and of which it was with little doubt the precursor or ancestral type. Thus the Palæoblattariæ are more nearly allied in the ground structure of their wings to certain neuropteroid Palæodictyoptera of paleozoic times than to the modern Blattariæ; and yet we can so completely trace in mesozoic times the transition from the Palæoblattariæ to the Blattariæ, that no reasonable doubt can exist as to their descent, the one from the other.

3. The ordinal distinction which is now found in the wing structure of modern insects did not exist in paleozoic insects, but a common simple type of neuration which barely admitted of family division.

It will appear from this that, by a sort of principle of family continuity, we may recognize in the paleozoic insects a tendency toward a differentiation in ordinal characters, sufficient to enable us in an *ex pos facto* fashion to distinguish between orthopteroid, neuropteroid, etc., Palæodictyoptera.

In speaking above of the different orders of Heterometabola which were foreshadowed in ancient times, I included the Coleoptera with a limitation, for the following reasons: Troxites, the only supposed paleozoic beetle which has not been shown to be an arachnid, is a very obscure object, and is very likely, as Brongniart has suggested, to be merely some fruit. But there have been found wood borings of different kinds which so nearly resemble similar excavations made now by Coleoptera, that it is natural, though of course not necessary, to substitute these for them. Yet if Coleoptera, with front wings differentiated as those of to-day, existed then, it would be rather anomalous, since all the paleozoic insects we know excepting one, Phthanocoris, which foreshadowed the heteropterous Hemiptera, had fore wings as completely membraneous as the hind wings.

It seems to me probable, therefore, though there are no further grounds for it than those just given, coupled with the present relationship of the Coleoptera to other Heterometabola, that Coleoptera sprang from such Palæodictyoptera as were wood-borers throughout the greater part of their life, and which at first showed no greater distinction between the front and hind wings than existed generally in other Palæodictyoptera; but afterward those races were preserved in which the thickening of the membrane of the upper wings the better protected the insects while in their burrows for the marriage flight in open air. Their habits would render their preservation in the rocks less frequent, and this special differentiation would be likely to proceed rapidly, and to be retained even by those which lost the wood boring habit—a habit, by the way, likely to have existed with some insects living in vast carboniferous forests.

Of the metamorphoses of the paleozoic insects we know absolutely nothing, for no larval or pupal form has yet been found, nor even any apterous insect which might by any possibility be looked upon as such. The preparatory stage of existing Heterometabola; the fact that from every form of evidence the more "complete" metamorphosis must have been derived from the less complete; and the generally admitted proposition of Brauer and others that metamorphosis, that is, radical change of form after birth, is a secondary adaptive feature—these all lead us to conclude that the only significant change in the paleozoic Palæodictyopteron after leaving the egg was the acquirement of wings; and that the acquirement of wings was the lever which natural selection handled to procure the present varied forms of metamorphosis in insects.

A curious and somewhat unexpected fact is found in the present universal prevalence of membraneous front wings in all the orders of Metabola, similar to what is found in the direct paleozoic ancestors of Heterometabola; while most existing Heterometabola, though lower in general organization than the Metabola, have passed beyond this feature of uniformity to one of greater differentiation, the front wings being more or less coriaceous, while the hind wings are still membraneous. This, together with the direct relation of some paleozoic insects to later types, would lead us to believe that we are to look at the neuropteroid Palæodictyoptera as the ancestors not only of later Neuroptera but also of all Metabola, and would account in a measure for the somewhat close relationship of the Phryganidæ and lower Lepidoptera.

Allusion has been made to Brongniart's discovery of an insect's wing in the middle Silurian—a long way removed from the upper Devonian, which had hitherto been their lowest known horizon. But though he quickly published a rude figure of his fossil, it is insufficient for critical purposes, and it would probably be hard to obtain from a single discovery the clew we need as to the ancestry of the Palæodictyoptera. We may safely conclude, however, that the winged Palæodictyoptera came in as early as the middle Silurian, and that up to the close of the paleozoic epoch their divergent stems were still admissible into one general order.

Now, when we look at the insects of later formations, we find types of every one of the existing orders of insects—speaking of these orders in their broadest sense, as we have everywhere done in this essay; we find every one fully developed in the Jurassic period.

In the Orthoptera we find as good a proof as anywhere, since cockroaches are the only insects found in any numbers in the very lowest mesozoic rocks. Their presence in the Trias and its significance will be alluded to later. In the Jurassic rocks nearly forty species are known, of which about one-third are in the lower Jurassic, and nearly all are true Blattariæ. So, too, in the Liassic rocks we recognize all the families of saltatorial Orthoptera and the Forficulariæ, so that the Orthoptera may be considered as well established early in mesozoic times. Unfortunately, no Phasmida have yet been recovered.

Only one or two Neuroptera have been recognized in the Trias, but in the Lias we have a considerable number, including Megaloptera, Sialina, Panorpidæ, Phryganidæ, Ephemeridæ, Termitina, and Odonata, showing that the differentiation into the non-existing families was apparently complete early in mesozoic times, and that forms of nearly all recognized families were abundant in the middle and later Oolite.

The two orders just mentioned are almost the only ones that have yet been recognized in the scanty fauna of the Trias, but the moment we reach the lower Jurassic rocks we find traces of nearly all the others; thus several families both of Homoptera and of Heteroptera are found in Liassic rocks, including such diverse types as the Coreidæ, Belostomidæ, Cicadina, and Cicadellina, while Fulgorina and Aphidina are added in the Oolite.

The Coleoptera, of which we found only indefinite traces in paleozoic rocks, have been found in the Trias (Chrysomelites), and the adjacent Rhaetic has disclosed forms as different as Hydrophilites, Buprestites, and Curculionites, while the Lias already claims some one hundred and twenty-five species referred to as many as seventeen distinct families.

When we come to the metabolous orders, we find a scantier representation, but in the more limited sense necessarily attendant upon this fact nearly the same things are true. Three or four species of Diptera, referred to Chironomidæ, Tipulidæ, and Asilidæ, are found as low down as the Lias, about as many more in the middle Oolite, and some fifteen or twenty in the upper Oolite, of several different families, mostly Nemocera. Of Lepidoptera, the remains of which are exceedingly scanty even in the tertiaries, we know of two, unquestionable Sphingidæ in the middle Oolite, and the mines of a tineid moth in the Cretaceous. While of Hymenoptera we have eight or ten mesozoic species, the oldest of which is an undoubted ant from the Lias, next a wood wasp and four or five very obscure remains from the middle Oolite of Solenhofen, two ants again from the upper Oolite (Purbecks), and the eggs of one of the Tenthredinidæ from the Cretaceous.

We find, then, that the entire change from the generalized hexapod to the ordinarily specialized hexapod was made in the interval between the close of the paleozoic period and the middle, we may say, of the mesozoic. These significant changes were ushered in with the dawn of the mesozoic period, and the Triassic rocks become naturally (together with the Silurian) the most important, the expectant, ground of the student of paleontology. Hitherto for fifty years the Carboniferous period has claimed this interest as its birthright.

The Silurian period has furnished only a single insect, just discovered and already alluded to. The Triassic has four or five representatives in the Old World, while a new locality recently made known in Colorado has yielded a considerable number of specimens of about twenty species, mostly still unpublished. Most of these are cockroaches, and they illustrate and enforce the conclusion we have reached in an interesting way. One of them, the European Legnophora of Heer, shows for the first time in the history of cockroaches a thickening of the front wings, rendering the veins nearly obsolete, a characteristic of Blattariæ (not always very striking), but never found in Palæoblattariæ. A similar appearance is to be seen in a few of the American cockroaches of the Trias, and in addition to this they are divided between Blattariæ and Palæoblattariæ, and the passage from one to the other is traceable. The two exist side by side, but some of the Blattariæ have the front wings equally membraneous.

It would, then, appear that the geological history of winged insects, so far as we know from present indications, may be summed up in a very few words. Appearing in the Silurian period, insects continued throughout paleozoic times as a generalized form of Heterometabola which for convenience we have called Palæodictyoptera, and which had the front wings as well as the hind wings membraneous. On the advent of mesozoic times a great differentiation took place, and before its middle all of the orders, both of Heterometabola and of Metabola, were fully developed in all their essential features as they exist to-day, the more highly organized Metabola at first in feeble numbers, but to-day and even in tertiary times as the prevailing types. The Metabola have from the first retained the membraneous character of the front wings, while in most of the Heterometabola, which were more closely and directly connected with paleozoic types, the front wings were, even in mesozoic times, more or less completely differentiated from the hind wings, as a sort of protective covering to the latter, and these became the principal organs of flight.

Another paper was by Prof. E. D. Cope, of Philadelphia, on the Pretertiary Vertebrata of Brazil. He stated that the tertiary vertebrata of Brazil, and of South America, north of Patagonia, and of Central America and Mexico as far north as the city of Mexico, as known up to the present time, belong to one fauna and one geological horizon, the Pampean or Pliocene. The diversity of this fauna from the contemporary faunæ of other regions of the earth has been shown to be great. It is one of the points to be determined by the study of earlier faunæ, whether the same diversity existed at earlier periods of the history of the South American continent.

The speaker stated that he had received collections from the Brazilian Museo Nacional, through Prof. O. A. Derby, the director, from localities from Clara on the north to the southern part of the province of San Paolo on the south. The fossils are referable to Cretaceous, Jurassic, and probably Permean formations. Certain fishes from the fresh water beds near Bahia have a decidedly lower tertiary or Laramie character. From beds of the same region, bones of dinosaurian reptiles had been procured. From marine Cretaceous beds came crocodiles and fishes of the same genera as those found in the upper Cretaceous of New Jersey, but of distinct species. One of the fishes, a Pycnodus, gave important evidence as to the true position of that genus in the system. The most important fossil of the Permian beds is a probable reptile of very primitive type, whose position is yet uncertain. It had the ribs fixed immovably to the vertebral bodies, and was thus incapable of breathing by intercostal movements. The feet indicate natatory habits; and the speaker inferred that the fixity of the ribs was connected with a generally subaqueous habitat. It was named *Stereosternum timidum*. The form is not like any genus hitherto known from the Permian period.

Prof. Theodore Gill read a paper On the Orders of Fishes. He said that the system of ichthyology is the reproach of modern science. Prof. Cope has contributed most to the advancement of systematic ichthyology. The word "fish" as ordinarily used means simply those vertebrate animals that live in water, and has no scientific value. The term "teleostomes" is proposed instead of "fish" to designate the true fishes. There are 8 or 9 extinct orders. The following scheme exhibits the classification proposed by Prof. Gill. It is to be noted that so-called "ganoids" are not mentioned.

1. Leptocardians.
2. Myzonts:
   - Hyperotreti.
   - Hyperoartii.
3. Selachians:
   - Holocephali.
   - Opistharthri.
   - Proarthri.
   - Anarthri:
     - Galei.
     - Rhinæ.
   - Platysomi.
4. Teleostomes:
   - Dipnoi.
   - Crossopterygi.

Teleostomes—Continued:
   - Selachostomi.
   - Chondrostei.
   - Rhomboganoidei.
   - Cycloganoidei.
   - Plectospondyli.
   - Nematognathi.
   - Gymnonoti.
   - Scyphophori.
   - Symbranchia.
   - Carenchelyes.
   - Apodes.
   - Lyomeri.
   - Opisthomi.
   - Xenomi.
   - Iniomi.

Prof. Cope approved the omission of ganoids as a distinct group, and discussed some details of classification.

Prof. Gill replied that he considered selachians and teleostomes distinct. In selachians, we have ossification, but not membrane bones; but in teleostomes we have bony fins, sustained by true membranes.

We have thus a great advance in the true fishes. All the membrane bones are developed in them, and are developed for the first time in them.

Prof. Cope insisted on a double cross classification proposed by him, in which the presence or absence of membrane marked one line of classification, and that of a quadrate bone the other, giving membraneous fishes with and others without a quadrate; also non-membranous fishes with and others without quadrate, thus:

	No Quadrate.	Quadrate.
Membraneous......	Holocephali.	Selachians.
Non-membraneous,	Dipnoi.	True fishes.

The Classification of Natural Silicates, by T. Sterry Hunt.—The author noticed the so-called natural history method of classification of mineral species, based on physical characters only, and also the purely chemical method, which treats them as chemical compounds without regard to physical characters. He contended that a true and natural classification must keep in view both chemical and physical relations, and proceeded to set forth a new system, which was illustrated by showing its application to all natural silicates as set forth in three printed tables. In these the silicates are divided into protosilicates, protopersilicates, and persilicates, in each of which the species are arranged with regard to the oxygen ratios between silica protoxides and sesquioxides, and also with reference to the molecular condensation, or in other words, the spatial relation of the oxide unit, which is shown to be closely connected with hardness, specific gravity, and chemical characters. The protopersilicates of varying oxygen ratios, for example, are grouped in separate columns under the heads of zeolitoid, spathoid, adamantoid and phylloid, a subordinate division of the latter being called pinitoid. In like manner the protosilicates are arranged as pectoloid, spathoid, admantoid, and ophitoid, and the persilicates as kaolinoid and adamantoid. This, it was claimed, gives a natural system for mineralogy.

Prof. G. W. Hill read a paper on a discussion of the two lunar inequalities due to the action of Jupiter, whose discovery is due to Mr. E. Neison. One of them had been previously pointed out by Prof. Newcomb.

Mr. Neison gives $-1''.163$ and $+2''.200$ as the coefficients of these inequalities. Prof. Hill finds $-0''.903$ and $+0''.209$, showing some discrepancy, especially in the latter coefficient.

In his paper on the Gampsynochidæ, an Undescribed Family of Fossil Schizopod Crustacea (one of several papers on crustaceans read by him), Prof. Packard remarked that from the form described the higher crustaceans and decapods may have arisen. It was similar to some of the specimens brought up from deep sea by the dredgings of the Challenger.

Prof. C. A. Young read a paper on Recent Observations upon the Rotation and Surface Markings of Jupiter, for the purpose of calling attention of other observers, and increasing the number of observations.

Ten years ago a red spot appeared on the surface of Jupiter, which became, and long continued to be, the most conspicuous object on that planet. The central portion of it is now covered with a white patch.

Late in March of this year, he noticed a long row of white spots in the southern hemisphere, above latitude 40° to 45°. He selected for observation one which was readily distinguished by its position, being the southern in a quadrilateral, and having a diameter of 1° to 1½°. His observations were correct within one minute, and give the following results, showing a decrease of over five minutes in the time of Jupiter's diurnal revolution as compared with the results deduced form obsevations of the red spot.

	h. m. s.	h. m. s.
March 25........	9 24 00 Sid.	9 09 24·3 M. T.
April 1..........	10 30 30	9 48 02·2
	Interpolated	9 48 00·9
April 18.........	10 21 00	8 31 48·8

	h. m. s.
Rotation time, 58 revs.	9 55 12·74
Ten years ago	9 55 54
Red spot	9 55 38·4
Equatorial white spot	9 50 9 to 12

Mr. A. N. Skinner (by invitation) read a paper on the total solar eclipse of August 28, 1886, in which he presented facts bearing on the advisability of attempting observations.

The eclipse will begin at sunrise near the east coast of South America, where observation will be impossible, owing to nearness of sun to the horizon, and will extend across the Atlantic to the coast of Africa, near Benguela.

There are no islands in the Atlantic near the place of maximum duration of the eclipse. Hence, the only possible observing stations must be in Africa. The duration of totality in mid-ocean is 6 m. 33 s.; on African coast, 4 m. 41 s. This is long enough to give useful results if it proves practicable to establish an observing station. The time of day will be favorable, being about 3 P. M., when the sun will be about 40° high.

This portion of the coast is very unhealthy, but the land rises rapidly, and it is more healthy a few miles inland.

Access to the interior is, however, difficult and dangerous.

The shadow of the eclipse is 116 miles in diameter where it strikes the coast. The axis of the shadow is near a good harbor, though the water supply is poor. The nearest settlement is Benguela, 20 to 25 miles distant. There are two monthly lines of steamers to Benguela. One plies from Lisbon, the other from Liverpool.

It was suggested that possibly an observing party might be stationed aboard a United States naval vessel anchored in the harbor; the observers doing work ashore in the daytime, and sleeping aboard at night, which would be some protection from miasm.

The eclipse occurs near the close of the dry season, which is the most healthy portion of the year, and gives good probability of clear sky. The line of vision being seaward would obviate obstruction of smoke from burning brush, etc., from land fires, which are then prevalent.

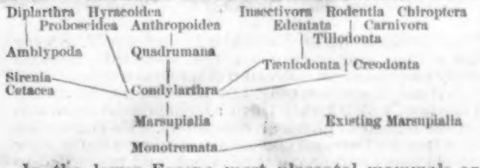
Prof. E. D. Cope read a paper on the Phylogeny of the Placental Mammals. He supposes all existing mammals to have arisen from monotremes through the intervention of extinct forms of marsupials, though he admits some doubt about cetaceans. This result is not obvious at first, for the reason that of the three divisions of mammals, the placental mammals are now widely differentiated from monotremes and marsupials, neither of which are placental. The marsupials were specialized early, and have even lost ground later.

The phylogeny of the mutilates is not well understood.

The rest of the placentals are divided into ungulates and unguiculates.

He presented the following scheme of phylogeny:

PHYLOGENY OF MAMMALS.



In the lower Eocene, most placental mammals are referable to two types, one ungulate, the other unguiculate. They had a general identity in osteology, and were only distinguished by the phalanges (*i. e.*, into hoofed and clawed animals).

The primitive unguiculate presents a form similar to a carnivore of the creodont group.

Man belongs to the ungulate (or hoofed) series.

The classification of mammals now adopted by Prof. Cope is shown in the following scheme:

I. A large coracoid bone articulating with the sternum.

Marsupial bones: fibula articulating with proximal end of astragalus.

1. Monotremata.

II. Coracoid a small process co-ossified with the scapula.

α. Marsupial bones: palate with perforations (vagina double; placenta and corpus callosum rudimental or wanting; cerebral hemispheres small and smooth).

But one deciduous molar tooth.

2. Marsupialia.

αα. No marsupial bones: palate entire (one vagina; placenta and corpus callosum well developed).

β. Anterior limb reduced to more or less inflexible paddles, posterior limbs wanting (mutilata). No elbow joint; carpals discoid, and with the digits separated by cartilage; lower jaw without ascending ramus.

3. Cetacea.

An elbow joint; carpals and phalanges with normal articulations; lower jaw with ascending ramus.

4. Sirenia.

ββ. Anterior limbs with flexible joints and distinct digits; ungual phalanges not compressed, and acute at apex (ungulata).

γ. Tarsal bones in linear series; carpals generally in linear series.

Limbs ambulatory; teeth with enamel.

5. Taxeopoda.

γγ. Tarsal series alternating; carpal series linear. Cuboid bone partly supporting navicular, not in contact with astragalus.

6. Proboscidea.

γγγ. Both tarsal and carpal series more or less alternating. Os magnum not supporting scaphoides; cuboid supporting astragalus; superior molars tritubercular.

7. Amblypoda.

Os magnum supporting scaphoides; superior molars quadri-tubercular.

8. Diplarthra.

βββ. Anterior limbs with flexible joints; ungual phalanges compressed and pointed (unguiculata).

ε. Teeth without enamel; no incisors.

Limbs not volant; hemispheres small, smooth.

9. Edentata.

εε. Teeth with enamel; incisors present.

No postglenoid process; mandibular condyle round; limbs not volant; hemispheres small; smooth.

10. Rodentia.

Limbs volant; hemispheres small; smooth.

11. Chiroptera.

A postglenoid process; mandibular condyle transverse; limbs not volant; no scapholunar bone; hemispheres small; smooth.

12. Bunotheria.

A postglenoid process; limbs not volant, with a scapholunar bone; hemispheres larger. Convoluted.

13. Carnivora.

In the discussion that followed, Prof. Morse expressed great admiration for Prof. Cope's work, especially for the directness of the passage shown from man to the lower animals of the Eocene.

Prof. Gill disliked the term quadrumana, preferring to classify the so-called quadrumana with the anthropoids, calling both primates. The condylarthra should be separated from the primates. The freedom of limbs from body in primates is due to arboreal habits. Simiæ and primates are so closely allied that their separation must have been recent. Man is derived from a genus like troglodytes—some form like the gorilla or chimpanzee. The cercopitheci should be excluded from the anthropoid group.

Prof. Cope explained his system more in detail, showing substantial argreement with the main points suggested by Prof. Gill.

Prof. Morse inquired whether the separation of anthropoids was not earlier than the Miocene.

Prof. Cope replied that the Hominidæ are more recent, but the anthropoid group has a more primitive origin. He would derive the group from some extinct lemus, which in turn came direct from condylarthra.

The Evolution and Homologies of Flukes of Cetaceans and Sirenians was presented by Profs. Theodore Gill and John A. Ryder.

Prof. Gill began by stating that three versions had been given of the relations of flukes to corresponding parts of other mammals:

1. That of Linnæus, that the flukes correspond to the hinder limbs of quadrupeds.

2. Flower's theory that the flukes represent lateral expansions of an ordinary tail; and

3. Gill's own proposition that the hinder limbs of ordinary quadrupeds are partly represented by flukes and partly by small bones developed in some cetaceans. These alone represent the pelvis and limb bones. The fluke represents the integuments of ordinary feet. Thus we have atrophy of the osseous corresponding with excessive hypertrophy of the tegumentary fabric.

Prof. Ryder followed with a presentation of results of his investigations.

The embryo of certain osseous fishes exhibits a translocation within the period of development ranging from 24 to 48 hours. The morphology of seals is instructive in indicating the method of development. The development of bones in embryos generally is inverse to the progress of atrophy.

These studies appear to demonstrate that cetaceans have descended from terrestrial mammals having well developed ambulatory limbs.

Prof. Ira Remsen read a paper on Chemical Action in a Magnetic Field. He has been experimenting for two or three years, in order to ascertain whether magnetized iron conducts itself differently from non-magnetized in the presence of chemical reagents. The deposit from a solution of copper upon iron on a magnet is very curious in its arrangement. It appears as if those parts of iron which are highly magnetized are less affected than others by chemical action. He put iron on glass above a magnet, and poured on nitric acid with abnormal results.

Instead of an electro-magnet, he then tried the action of nitric acid on iron in a test tube. The chemical action in a tube placed in a coil is retarded. This fact is fully established.

The paper gave rise to animated discussion by Profs. Newcomb, Barker, Bell, Young, and Wright. In reply to questions Prof. Remsen said that no effect had been discovered in action of non-magnetizable substances; also that the current had been always the same way, so that no comparison had been made between positive and negative poles.

Prof. A. Graham Bell read a paper on the Measurement of Hearing Power, giving results of recent experiments by him on 727 children of the Franklin School, Washington. These results are thus tabulated:

Audiometer readings, in centimeters.	Number of pupils.	Percentage.
0 to 10		
10 " 20		Obtuse.
20 " 30	1	0·3
30 " 40	15	2·0
40 " 50	57	7·8
50 " 60	176	24·2
60 " 70	227	31·2
70 " 80	202	27·8
80 " 90	49	6·7
		Acute.
Total	727	100·0

The audiometer employed is a telephone. We produce a sound of known intensity by the rotation of Siemens' armature. The distance between two coils enables us to calculate the degree of loudness of the sound. The patient draws the coils apart, thus diminishing the intensity of the sound till it becomes inaudible to him, and the number of centimeters in the above table marks this distance. This test, therefore, gives the result of the best ear. There is almost always a difference between ears.

Partial deafness is very prevalent among children. In New York it was found that ten per cent. of the children were hard of hearing, and the above table shows nearly the same result, all below 50 centimeters being considered as below par. There is no broad line of demarkation between deaf and hearing people, but all shades of deafness and hardness of hearing are found.

Prof. Scudder inquired the explanation of the sudden jump between 50 and 60.

Prof. Bell conjectured that it might be partly explained on the hypothesis that children who could not hear at a greater distance of the coils than 50 centimeters were known to be hard of hearing, and were not sent to school for that reason.

Prof. Bell followed with a paper on the Possibility of obtaining Echoes from Ships and Icebergs in a Fog.

He detailed experiments by Mr. F. Della Torre at Baltimore during the session of the Academy, which had been witnessed by Prof. Bell.

They went to sea, and fired blank cartridges at approaching vessels, placing hearing tubes (speaking trumpet) upon their guns. At the distance of a mile they could perceive an echo. Even from a small tug, end on, they perceived an echo at the distance of a quarter of a mile.

Mr. Della Torre proposes to put guns on the bow of vessels, and explode cartridges, which would give an echo, and enable the presence of obstructions to navigation to be distinguished in a fog.

One consideration may militate somewhat against the success of this method in rough weather. The waves may produce a sound that will confuse the experimenter. It was noticed that when the water was rough there was a sound like rumbling thunder. As they were several miles from shore, and the sound came back almost immediately, where there were no vessels near, but the waves were high, this sound must have been caused by the echo from the water itself.

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